THE ACCOUNTING REVIEW

VOLUME 82, No. 3 MAY 2007

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The Accounting Review (ISSN 0001-4826 print and ISSN 1558-7967 online) is published five times a year in January, March, May, July, and October by the American Accounting Association, 5717 Bessie Drive, Sarasota, Florida 34233-2399. General subscriptions are available at the annual rates of $315 for a print copy or at $350 for a print and electronic copy. The member subscription rate for a hard-copy is $20. Electronic access is included with membership dues. Periodical postage paid at Sarasota, Florida and additional mailing offices. **POSTMASTER:** Send address change to *The Accounting Review,* 5717 Bessie Drive, Sarasota, Florida 34233-2399.

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THE ACCOUNTING REVIEW
Vol. 82, No. 3
2007
pp. 551–580

Interacting Supply Chain Distortions: The Pricing of Internal Transfers and External Procurement

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ABSTRACT: This paper examines the impact of distortions wrought by transfer pricing when a firm is engaged in both internal production and external procurement of inputs. In particular, we demonstrate that a firm can actually glean benefits from often-discussed transfer-pricing problems in dealing with an external supplier. Though transfer prices above marginal cost introduce inter-division coordination problems, they also introduce a lower willingness to pay to outside suppliers. Knowing that costly internal transfers will eat into demand, the supplier is more willing to set lower prices. Such supplier discounts can make decentralization worthwhile for the firm. This benefit of decentralization is shown to be robust to variations in both downstream and upstream competition.

Keywords: *decentralization; procurement; transfer pricing.*

I. INTRODUCTION

Firms routinely confront supply chain frictions. A common issue is that of a firm dealing with production distortions wrought by an over-charging supplier.[1] In an internal accounting context, conflicts of interest between upstream supply divisions and downstream procuring divisions have firms struggling to determine effective transfer-pricing methods. In this paper, we consider a firm engaged in both internal production and external procurement of inputs, so divisional conflicts and aggressive suppliers are each possibilities. Such simultaneous consideration of multiple supply chain frictions conveys a

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[1] See Tirole (2003, Ch. 4) for a concise review of such issues in vertical supply chains. The role of information exchange, and the related issue of potential information misappropriation, among supply chain partners is the focus in Baiman and Rajan (2002).

We thank Mark Bagnoli, John Fellingham, Paul Fischer, Larry Gordon, Michael Kirschenheiter, Martin Loeb, Henock Louis, Stefan Reichelstein, Doug Schroeder, Shyam Sunder, Ro Verrecchia, Susan Watts, Rick Young, seminar participants at the University of Maryland, The Pennsylvania State University, and Purdue University, and especially Madhav Rajan (editor) and the anonymous referees for helpful comments. Professor Arya gratefully acknowledges support from the John J. Gerlach Chair.

Editor's note: This paper was accepted by Madhav Rajan.

Submitted April 2006
Accepted October 2006

different story than that of a firm trying to stifle all internal inefficiencies: modest internal frictions can sometimes serve as a natural brake on exploitative external parties.

In particular, the paper demonstrates that when a firm relies on externally supplied inputs, it can benefit from often-discussed transfer-pricing problems. Though transfer prices above marginal cost introduce coordination problems, they also introduce a lower willingness to pay to outsiders. The resultant discounts from suppliers may make such internal strife (and, as a result, decentralization) worthwhile.

To elaborate, we consider a model in which a firm produces a product that requires two inputs. In line with the tendency of firms to keep some aspects of business in-house while outsourcing others, we assume one input is produced internally while the other is purchased from a supplier. Relying on a self-interested supplier introduces frictions in the supply line: the supplier charges a premium for its product, thereby putting the firm in a position where it restricts procurement relative to that of a vertically integrated entity. This is the usual problem of double-marginalization. A move toward decentralization of internal production introduces a second friction that also falls under the rubric of double-marginalization: transfer prices above marginal cost for the internal good further increase the cost of production from the standpoint of the procuring (downstream) division and thus further cut into production.

While introducing a second problem of excessive input prices may seem counterproductive, it turns out that such a curse can be a blessing in the face of an external supplier. Knowing that costly internal transfers will cut into demand, the supplier is more willing to set lower prices. However, as stressed above, decentralization and transfer pricing are not without their downside. Inefficient production is a key consequence. Moreover, if the firm faces competition in selling its product, such inefficiencies can also erode competitive position. Nonetheless, these costs can be outweighed by the benefit of eliciting favorable supplier terms.

The essential tensions of this paper's model persist under both quantity and price competition in downstream markets. While the competitive loss from decentralization in the setting is similar across regimes, the detrimental consequences are less pronounced under price competition. To further investigate the robustness of the results, we vary the relative bargaining power of the firm's divisions in setting transfer prices. We show that the basic conclusions persist regardless of the particular assignment of bargaining power. Moreover, if the firm can actively influence decision-making authority (bargaining power) of the divisions, decentralization can actually replicate the transfer price the firm would have chosen could it credibly precommit to one.

We also consider the effects of multiple input suppliers as well as multiple input customers on the analysis. Besides demonstrating that the key results persist under different supply market scenarios, these modeling variations provide suggestive comparative statics. In particular, circumstances in which one would expect upstream pricing considerations to drive benefits of decentralization include limited input market (upstream) competition, restrained product market (downstream) competition, and mutual reliance on a supplier by competing firms.

This is not to say that firms decentralize with the sole intention of cutting input prices. Rather, the broader point here is that decentralization can have far-reaching effects: it not only alters the internal operations and competitive posture of a firm, but it can also influence the behavior of other constituents (in this case, suppliers). As such, the paper suggests that one reason the common practice of decentralization (and tolerance of modest internal frictions) may survive is because of its ability, even if unintentional, to soften aggressive suppliers. Thus, upstream interactions may be a factor to consider in examining empirical determinants of corporate structure and compensation decisions. In a similar vein, explicit consideration of downstream interactions has helped explain the limited use of relative performance evaluation in practice (Aggarwal and Samwick 1999).

A vast literature has detailed pricing forms and the ensuing distortions that can plague decentralized firms.[2] This paper is most related to the literature detailing strategic benefits of such decentralization-induced distortions (e.g., Fershtman and Judd 1987; Hughes and Kao 1998) and, in particular, strategic transfer pricing (Alles and Datar 1998; Goex 2000; Narayanan and Smith 2000). In these papers, decentralization allows a firm to reap benefits in horizontal (competitive) relationships. In Fershtman and Judd (1987), decentralization of decisions and particular compensation structures allow a firm to commit to aggressive quantity competition, thereby convincing a rival to back down. In Hughes and Kao (1998) and Alles and Datar (1998), decentralization allows for tacit coordination under price competition among firms supplying in several markets. Goex (2000) and Narayanan and Smith (2000) also show coordination benefits—by setting transfer prices above marginal cost, firms effectively commit to softer price competition.

In this paper, strategic benefits of decentralization arise not in downstream competitive relationships but rather in upstream interactions. In fact, downstream competition only dampens benefits of decentralization. The benefit of decentralization does not come from strategic coordination (the upstream supplier stands to lose from decentralization) nor does it come from the firm wanting to convey a position of strength. Rather, the firm benefits from decentralization because it conveys weakness, which, in turn, forces the upstream supplier to reduce its price. In this sense, the underlying force in this setting is most closely tied to the notion of self-sabotage (Sappington and Weisman 2005).

Sappington and Weisman (2005) demonstrate that self-sabotage (in the form of inflated costs) can prove beneficial in the competitive realm. In a similar vein, this paper demonstrates that apparent reductions in firm efficiency can have positive repercussions in vertical relationships. A distinguishing feature of the self-sabotage studied in our paper is that the sabotage takes place in accounting terms. That is, while inflating actual costs is itself not worthwhile, the firm benefits from inflated pseudo-costs (transfer prices) and the collateral effects on division and supplier behavior.

The paper proceeds as follows. Section II presents the basic setup. Section III details the main results, including a consideration of different forms of downstream competition and varying means of setting transfer prices. Section IV considers variations in the input supply market. Section V concludes.

II. THE BASIC SETUP

A firm (denoted firm 0) is engaged in Cournot competition with n, $n \geq 0$, other providers of differentiated goods in the sale of its product.[3] Firm i's product quantity is denoted q_i, $i = 0,\ldots,n$. Consumer demand for firm i's product is represented by a linear, downward-sloping (inverse) demand function $P_i = a - q_i - \sum_{j \neq i} kq_j$, where P_i is the retail price of firm i's product, and $k \in [0,1]$ represents the degree of substitution among products. For $k = 0$, the products are independent, while for $k = 1$, the products are perfect substitutes.[4]

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[2] Baldenius et al. (1999) and Baldenius et al. (2004) are notable examples of papers that contrast different transfer-pricing methods as well as confront multiple firm objectives in their analysis.

[3] The case of downstream monopoly corresponds to $n = 0$.

[4] Such a demand function arises when a representative consumer maximizes a quadratic utility function subject to a budget constraint (e.g., Hackner 2000; Singh and Vives 1984).

To make its product, firm 0 relies on one internally generated good and one good that is provided by a sole supplier. We normalize the production process such that one unit of the final product requires one unit of each intermediate good. The internally generated good is produced at a constant unit (marginal) cost c_I, and the externally generated good is produced by the supplier at a constant unit cost c_E, $c_E + c_I < a$. For simplicity, assume the marginal production cost of each competitor is $c_E + c_I$.

Given this basic setup, we investigate firm 0's profits under centralized and decentralized regimes. Under centralization, the firm maintains control over production decisions and seeks to maximize firm-wide profit. Under decentralization, an upstream division is provided decision-making authority over production and pricing of the internal intermediate good, while a downstream division is charged with conversion and sale of the final good. In this case, trade among divisions is governed by a transfer-pricing arrangement, and each makes decisions to maximize its own division's profit.

III. RESULTS

Supplier Pricing and Centralization

Consider first the outcome when the firm's decision making is not marred by internal strife (the firm is centralized). Since the firm has all pertinent information at its disposal, such a centralized approach is seemingly most efficient. The timeline under centralization is provided in Figure 1.

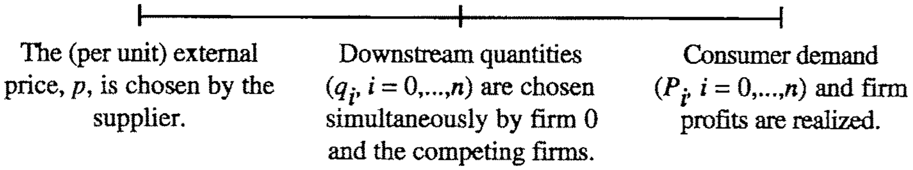
To determine the equilibrium under centralization, we work backward in the game. Given a supplier price p, the firm chooses q_0 to maximize its profit in the final product market. That is, firm 0 solves:[5]

$$\underset{q_0}{Max}\; q_0\left[a - q_0 - \sum_{j\neq 0} kq_j\right] - q_0[p + c_I]. \tag{1}$$

Similarly, firm 0's competitors, whose production cost per unit is $c_E + c_I$, solve:

$$\underset{q_i}{Max}\; q_i[a - q_i - \sum_{j\neq i} kq_j] - q_i[c_E + c_I],\ i \neq 0. \tag{2}$$

FIGURE 1
Timeline under Centralization



The (per unit) external price, p, is chosen by the supplier.

Downstream quantities (q_i, $i = 0,\dots,n$) are chosen simultaneously by firm 0 and the competing firms.

Consumer demand (P_i, $i = 0,\dots,n$) and firm profits are realized.

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[5] Throughout the paper, the first-order approach is valid since second-order conditions are always met and ensuing prices and quantities are positive.

Jointly solving the first-order conditions of Equations (1) and (2) yields equilibrium quantities as a function of the supplier price in the centralized setup, denoted $q_i^C(p)$, $i = 0,\ldots,n$. Given these quantities, the supplier sets its price to solve:

$$\underset{p}{Max}\; q_0^C(p)[p - c_E]. \tag{3}$$

The first-order condition of Equation (3) with respect to p yields $p^C(k,n)$, the optimal price set by the supplier for the external good under centralization. Substituting $p^C(k,n)$ for p in $q_i^C(p)$ yields equilibrium firm production levels. Substituting these in Equations (1) and (2) yields profits for the firms, denoted $\Pi_i^C(k,n)$. Lemma 1 summarizes the outcome of this process (all proofs are provided in the Appendix).

Lemma 1: The equilibrium outcomes under centralization are:

$$p^C(k,n) = c_E + \frac{(a - c_E - c_I)(2 - k)}{2[2 + k(n - 1)]};$$

$$q_0^C(k,n) = \frac{(a - c_E - c_I)}{2(2 + kn)};\ q_i^C(k,n) = q_0^C(k,n)\left[\frac{4 + k(2n - 1)}{2 + k(n - 1)}\right],\ i \neq 0;$$

$$\Pi_0^C(k,n) = \frac{(a - c_E - c_I)^2}{4(2 + kn)^2};\ \text{and}\ \Pi_i^C(k,n) = \Pi_0^C(k,n)\left[\frac{4 + k(2n - 1)}{2 + k(n - 1)}\right]^2,$$
$$i \neq 0.$$

Not surprisingly, the supplier sets the price above marginal cost, and the firm responds by procuring less than what it would have had the supplier charged c_E. This is the familiar double-marginalization problem. As a consequence, compared to its rivals that obtain the input at marginal cost, firm 0 produces less ($q_0^C(k,n) < q_i^C(k,n)$), and obtains lower profit ($\Pi_0^C(k,n) < \Pi_i^C(k,n)$). Moreover, as expected, an increase in downstream competition (an increase in the homogeneity of products or an increase in the number of rivals) reduces the firm's profit. The firm's profit is highest when it enjoys monopoly power in the market for its product ($n = 0$).

Supplier Pricing and Decentralization

A commonly discussed downside of decentralized organizations is the transfer-pricing distortions that result. In particular, upstream divisions, bent on boosting their own performance, seek transfer prices above marginal cost. In response, downstream divisions, focused on their divisional bottom line, have incentives to underprocure upstream goods. In short, decentralized firms are themselves subject to double-marginalization problems.

It is tempting to think that the addition of a second double-marginalization problem can only make matters worse for the firm. However, this argument ignores interactions between the intra-firm and inter-firm supply channels: the impact of internal strife can be to soften external pricing. Stated more precisely, this section shows that the cost associated with the internal double-marginalization problem can sometimes be more than offset by the ensuing weakening of the external double-marginalization problem.

To capture the real effects of transfer-pricing arrangements, we model a simple and commonly studied transfer-pricing policy for firm 0: the upstream division announces a per unit transfer price, t, and the downstream division chooses the number of units, q_0 to purchase.[6] Given such an arrangement, the downstream division pays attention not only to the price of the external good, p, but also to the transfer price of the internal good, t. The timeline under decentralization is provided in Figure 2.

Again, we work backward in the game to determine the equilibrium. Given p and t, the downstream division chooses q_0 to maximize its division profit, solving:

$$\underset{q_0}{Max}\; q_0[a - q_0 - \sum_{j\neq 0} kq_j] - q_0[p + t]. \tag{4}$$

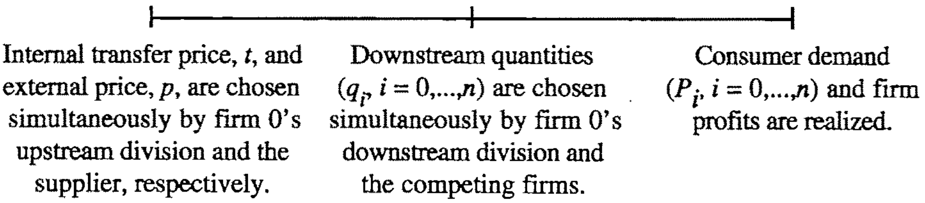
The problem for firm 0's competitors is as in Equation (2). Jointly solving the first-order conditions of Equations (2) and (4) yields equilibrium quantities as a function of the supplier price and the transfer price, $q_i^D(p,t)$, $i = 0,\ldots,n$. This serves as the induced demand function for the firm's upstream division and the supplier. The supplier and the upstream division simultaneously choose p and t, respectively, to maximize their own profits. The supplier's problem is in Equation (5), and the upstream division's problem is in Equation (6):

$$\underset{p}{Max}\; q_0^D(p,t)[p - c_E]. \tag{5}$$

$$\underset{t}{Max}\; q_0^D(p,t)[t - c_I]. \tag{6}$$

Jointly solving the first-order conditions of Equations (5) and (6) yields $p^D(k,n)$ and $t^D(k,n)$, the equilibrium pricing of the supplier and the upstream division, respectively. Substituting these in $q_i^D(p,t)$ yields equilibrium firm production levels. Finally, substituting these in Equations (1) and (2) yields profits for the firms, denoted $\Pi_i^D(k,n)$. Lemma 2 summarizes the outcome of this process.

FIGURE 2
Timeline under Decentralization



Internal transfer price, t, and external price, p, are chosen simultaneously by firm 0's upstream division and the supplier, respectively.

Downstream quantities (q_i, $i = 0,\ldots,n$) are chosen simultaneously by firm 0's downstream division and the competing firms.

Consumer demand (P_i, $i = 0,\ldots,n$) and firm profits are realized.

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[6] As Baldenius et al. (1999) note, this formulation is equivalent to standard cost transfer pricing in which there is no constraint on the upstream division's reported cost.

Lemma 2: The equilibrium outcomes under decentralization are:

$$p^D(k,n) = c_E + \frac{(a - c_E - c_I)(2 - k)}{3[2 + k(n - 1)]};\ t^D(k,n) = c_I + \frac{(a - c_E - c_I)(2 - k)}{3[2 + k(n - 1)]};$$

$$q_0^D(k,n) = \frac{(a - c_E - c_I)}{3(2 + kn)};\ q_i^D(k,n) = q_0^D(k,n)\left[\frac{6 + k(3n - 1)}{2 + k(n - 1)}\right],\ i \neq 0;$$

$$\Pi_0^D(k,n) = \frac{(a - c_E - c_I)^2[6 + 3k(n - 1) - k^2n]}{9[2 + k(n - 1)][2 + kn]^2};\ \text{and}$$

$$\Pi_i^D(k,n) = \Pi_0^D(k,n)\frac{[6 + k(3n - 1)]^2}{[2 + k(n - 1)][6 + 3k(n - 1) - k^2n]},\ i \neq 0.$$

As under centralization, the supplier sets price above marginal cost, and the downstream division responds by procuring less than what it would have had the supplier charged c_E. Similarly, the upstream division sets a transfer price above marginal cost, further depressing production levels. Thus, the problem of double-marginalization is magnified by decentralization. Again, the result is that firm 0 produces less than its rivals ($q_0^D(k,n) < q_i^D(k,n)$), and obtains lower profit ($\Pi_0^D(k,n) < \Pi_i^D(k,n)$). We next compare the outcomes under centralization and decentralization to determine the firm's preferred organizational structure.

Centralization versus Decentralization

At first glance, it appears that decentralization only does harm by magnifying concerns of double-marginalization. This view is borne out by noting firm 0's depressed production levels under decentralization, $q_0^D(k,n) < q_0^C(k,n)$. A mitigating factor is that the ensuing weakness of the firm forces the supplier to soften its pricing. In other words, by conveying internal strife, the firm convinces its supplier that it will be more sensitive to supplier pricing. The result is that $p^D(k,n) < p^C(k,n)$. Lower supplier pricing can actually make it beneficial to introduce an internal supply chain distortion in the presence of an external supply chain distortion. A comparison of $\Pi_0^D(k,n)$ and $\Pi_0^C(k,n)$ yields Proposition 1.

Proposition 1:

(i) If $n \leq 2$, then decentralization yields higher firm profit.
(ii) If $n > 2$, then decentralization yields higher firm profit if and only if $k < k^*(n)$, where:

$$k^*(n) = \frac{3(n - 1) + \sqrt{9 + 78n + 9n^2}}{8n}.$$

The proposition delineates that decentralization can be valuable, but only if competitive pressures are not excessive. Competitive considerations aside, though, it is not immediately apparent that the benefits of decentralization are sufficient. After all, even with no competition, the double-marginalization effect of decentralization leads to production levels that are below the firm's optimum. However, as the proposition notes, the benefit of decentralization can indeed exceed the cost. To intuitively address this trade-off, consider the monopoly case of $n = 0$.

In the absence of competition, a simple cost-benefit analysis succinctly captures the two effects of a decentralized structure. First, there is the cost of decentralization: transfer price above marginal cost artificially reduces production. Holding supplier price constant (at $p^C(k,0)$), the net cost to the firm of such production loss is:

$$\begin{aligned}&(q_0^C(k,0)[a - q_0^C(k,0)] - q_0^C(k,0)[p^C(k,0) + c_I])\\ &- (q_0^D(k,0)[a - q_0^D(k,0)] - q_0^D(k,0)[p^C(k,0) + c_I]) = \frac{(a - c_E - c_I)^2}{144}.\end{aligned} \tag{7}$$

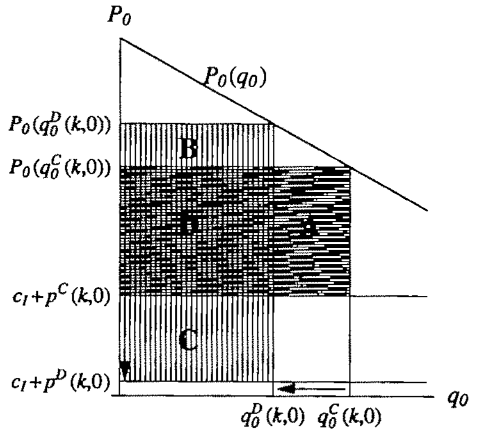
The offsetting benefit of decentralization is that a fractionalized firm convinces the supplier of a lower willingness to pay. In particular, holding production level constant (at $q_0^D(k,0)$), savings from a lower supplier price amount to:

$$(p^C(k,0) - p^D(k,0))q_0^D(k,0) = \frac{(a - c_E - c_I)^2}{36}. \tag{8}$$

Clearly, the benefit in Equation (8) exceeds the cost in Equation (7). In particular, the difference between Equations (8) and (7), $\frac{(a - c_E - c_I)^2}{48}$, is precisely the difference between $\Pi_0^D(k,0)$ and $\Pi_0^C(k,0)$. In effect, the firm benefits from a sort of "self-sabotage" in that it introduces inefficiencies that prove helpful (e.g., Sappington and Weisman 2005). However, the firm falls short of real sabotage, instead opting for "paper sabotage"—directly increasing costs is counterproductive, but increasing the internal accounting charge proves useful.

Figure 3 provides a summary of the cost-benefit analysis underlying the benefit of decentralization when $n = 0$. In the figure, the area A + D = $\Pi_0^C(k,0)$; the y-axis coordinates

FIGURE 3
Cost and Benefit of Decentralization under Downstream Monopoly



The vertical axis reflects output prices and input costs under centralization and decentralization.
The horizontal axis reflects quantities under centralization and decentralization.
The region shaded by horizontal lines (A + D) is firm profit under centralization.
The region shaded by vertical lines (B + D + C) is firm profit under decentralization.

correspond to the firm's contribution margin per unit and the x-axis coordinates reflect the number of units. Similarly, the area B + D + C = $\Pi_0^D(k,0)$. Hence, a shift from centralization to decentralization entails a change in profit of C − (A − B). Area C reflects the benefit of decentralization due to the supplier price falling from $p^C(k,0)$ to $p^D(k,0)$. Area (A − B) reflects the cost of decentralization due to the production level falling from $q_0^C(k,0)$ to $q_0^D(k,0)$; A is adjusted by B since restricted supply does provide an ancillary benefit to the firm of being able to obtain a higher price in the final product market.

Figure 3 also demonstrates that in the $n = 0$ case, the firm's benefit of decentralization comes with consequences for consumers. That is, the triangular area encompassed by the slanted consumer valuation line ($P_0(q_0)$) and the horizontal lines representing equilibrium prices in each regime ($P_0(q_0^C(k,0))$ and $P_0(q_0^D(k,0))$) is smaller under decentralization. In effect, the setting is one in which distortions in external supply beget internal distortions, and these distortions have deleterious consequences for consumers. In particular, it can readily be confirmed that decentralization reduces consumer surplus by:

$$\frac{(a - c_E - c_I)^2[20(1 - k) + kn(44 - 25k) + 5k^2(1 + kn) + k^2n^2(17 - 5k)]}{72[4 + 2k(2n - 1) + k^2n(n - 1)]^2} > 0.$$ [7]

As alluded to in the proposition, the added wrinkle brought by competition is that the firm's reduced production level under decentralization is an invitation for exploitation by competitors. This is borne out by the fact that competitor production levels are higher under decentralization than centralization, ($q_i^D(k,n) > q_i^C(k,n)$, $i \neq 0$). Further, the wedge between centralization and decentralization is more pronounced the higher k (i.e., $\frac{d[(q_i^D(k,n) - q_i^C(k,n))/q_i^C(k,n)]}{dk} > 0$). In effect, the greater the competitive pressure (higher k), the less attractive is decentralization, as shown in the proposition. Following this intuition, a greater number of competitors also reduces the attractiveness of decentralization. However, it does not preclude a net gain from decentralization.

Corollary 1:

(i) As the number of downstream rivals increases, the preference for decentralization declines, i.e., $k^*(n)$ is decreasing in n.
(ii) $\lim_{n \to \infty} k^*(n) = 3/4$. Thus, if $k < 3/4$, then decentralization is preferred to centralization for any n.

In effect, Corollary 1 highlights that while competitive effects are a consideration, they do not necessarily derail the upstream benefits gleaned from decentralization. At the core, the result suggests that not only are there strategic effects of transfer pricing in downstream interactions, but there can also be strategic effects of transfer pricing in upstream supply markets.

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[7] Total surplus is also reduced by decentralization; the difference in total surplus is:

$$\frac{(a - c_E - c_I)^2[76(1 - k) + kn(52 - 47k) + k^2(19 + 7kn) + 7k^2n^2(1 - k)]}{72[4 + 2k(2n - 1) + k^2n(n - 1)]^2} > 0.$$

As previous work on strategic transfer pricing stresses, benefits of decentralization can hinge on the issues of observability and commitment. In particular, it is often discussed that strategically set transfer prices are useful only if the firm can precommit to such prices.[8] In this setting, however, where strategic benefits arise in upstream markets, such precommitments are not crucial. To see this, consider the monopoly case. Here, there are only two relevant parties, firm 0 and the supplier, and transfer prices and supplier prices are jointly determined in equilibrium. Thus, the equilibrium requires no precommitment by the upstream division. In other words, since $p^D(k,n)$ and $t^D(k,n)$ are mutual best responses, the upstream division has no *ex post* incentive to change $t^D(k,n)$ once the lower supplier price, $p^D(k,n)$, is secured. What is important is that the firm can credibly establish its organizational structure. That is, once a firm decentralizes decision-making authority, it cannot unilaterally reclaim power and adjust transfer prices back to marginal cost.

A similar situation arises with the issue of observability. While observability of transfer prices is often the linchpin for strategic benefits to arise (e.g., Goex 2000; Narayanan and Smith 2000), only observability of organizational form is critical in this setup. Again, consider the monopoly case. Here, the timeline in Figure 2 demonstrates the effect of unobservability: since setting the transfer price before the supplier price is not needed to achieve benefits from the transfer price, it need not be observable to be useful. To highlight this, note that the equilibrium in which $p^D(k,n)$ and $t^D(k,n)$ are simultaneously determined best responses is equivalent to one in which the upstream division is a first mover but $t^D(k,n)$ is unobserved by the supplier (Bagwell 1995). What is essential to the result is that the supplier knows the upstream division is the party setting the price. The fact that observability of organizational structure is able to substitute for observability of transfer prices *vis-à-vis* upstream pricing is akin to the observability of compensation structures being sufficient for observability of allocation rules in Hughes and Kao (1998).[9]

Price Competition

It is often the case that implications derived under quantity competition are reversed under price competition (e.g., Bulow et al. 1985; Goex 2000). This section demonstrates that the paper's key conclusions, derived under quantity competition, also persist under price competition.

Consider the outcomes under price competition, given consumer demand as detailed under the basic setup, with $k < 1$. In this case, jointly solving the inverse demand curve equations yields equilibrium quantities as a function of prices:

$$q_i(P_0,\ldots,P_n) = \left[a(1-k) - (1+(n-1)k)P_i + \sum_{j\neq i} kP_j\right]/[(1-k)(1+kn)]. \quad (9)$$

---

[8] As Narayanan and Smith (2000, 513) note that "[i]f a firm can change its transfer price after the competition has observed it, even observable transfer prices will not play a strategic role because the firm cannot credibly commit to not doing something that is *ex post* optimal." In this vein, Narayanan and Smith (2000) show that tax considerations can provide an ancillary benefit of making strategically beneficial transfer prices *ex post* optimal.

[9] This is not to say, however, that observability of the transfer price does not affect downstream interactions. Formally, in solving the first-order conditions associated with Equations (2) and (4), p and t are taken as given. In choosing its production decision, each firm is influenced not only by its own cost, but also by the cost of its rivals, so whether the rivals' costs are observable influences the firm's output decision.

Given demand as in Equation (9), we next outline the equilibrium outcome under centralization and price competition. Given supplier price p, firm 0 chooses P_0 to maximize its profit in the final product market, solving:

$$\underset{P_0}{Max}\ q_0(P_0,\dots,P_n)[P_0 - p - c_I]. \tag{10}$$

Similarly, firm 0's competitors, whose production cost per unit is $c_E + c_I$, solve:

$$\underset{P_i}{Max}\ q_i(P_0,\dots,P_n)[P_i - c_E - c_I],\ i \neq 0. \tag{11}$$

Jointly solving the first-order conditions of Equations (10) and (11) yields downstream product prices under centralization as a function of the upstream supplier price, $P_i^C(p)$, $i = 0,\dots,n$. Given the ensuing quantities, the supplier sets its price to solve:

$$\underset{p}{Max}\ q_0(P_0^C(p),\dots,P_n^C(p))[p - c_E]. \tag{12}$$

The first-order condition of Equation (12) with respect to p yields $\tilde{p}^C(k,n)$, the optimal price set by the supplier for the external good under centralization (the tilde [~] reflects price competition). Substituting $\tilde{p}^C(k,n)$ for p in $P_i^C(p)$ then reveals equilibrium product prices that, when substituted in Equations (10) and (11), yield profits for the firms, denoted $\tilde{\Pi}_i^C(k,n)$.

The outcome under decentralization proceeds in a similar fashion. Given supplier price p and transfer price t, the downstream division chooses P_0 to maximize its division profit, solving:

$$\underset{P_0}{Max}\ q_0(P_0,\dots,P_n)[P_0 - p - t]. \tag{13}$$

The problem for firm 0's competitors is as in Equation (11). Jointly solving the first-order conditions of Equations (11) and (13) yields product prices under decentralization as a function of the supplier price and the transfer price, $P_i^D(p,t)$, $i = 0,\dots,n$. The demanded quantity brought by these prices serves as the induced demand function for the firm's upstream division and the supplier. The supplier and the upstream division simultaneously choose p and t, respectively, to maximize their own profits. The supplier's problem is in Equation (14), and the upstream division's problem is in Equation (15):

$$\underset{p}{Max}\ q_0(P_0^D(p,t),\dots,P_n^D(p,t))[p - c_E]. \tag{14}$$

$$\underset{t}{Max}\ q_0(P_0^D(p,t),\dots,P_n^D(p,t))[t - c_I]. \tag{15}$$

Jointly solving the first-order conditions of Equations (14) and (15) yields $\tilde{p}^D(k,n)$ and $\tilde{t}^D(k,n)$, the equilibrium pricing of the supplier and the upstream division, respectively. Substituting these in $P_i^D(p,t)$ yields equilibrium prices that, when substituted in Equations

(10) and (11) yield profits for the firms, denoted $\tilde{\Pi}_i^P(k,n)$. As detailed in the Appendix, a comparison of $\tilde{\Pi}_i^C(k,n)$ and $\tilde{\Pi}_i^P(k,n)$ yields the following proposition:

Proposition 2: Under price competition, decentralization yields higher firm profit for all k and n.

The proposition, coupled with Proposition 1, highlights that the benefit of decentralization in this paper can arise under both quantity and price competition. Importantly, supplier pricing effects are the sole impetus for decentralization under either form of competition, with competitive effects representing a net loss of decentralization. Under price competition, this is highlighted by the fact that an increase in the number of rivals reduces the net benefit of decentralization, i.e., $[\tilde{\Pi}_i^P(k,n) - \tilde{\Pi}_i^C(k,n)]$ is smaller for larger values of n.

Comparing the competitive regimes in Propositions 1 and 2, decentralization is even more attractive (preferred for a greater set of k and n values) under price competition. While the supplier pricing benefit and the double-marginalization cost of decentralization are both present in either competitive regime, the competitive cost varies across the two regimes. Because prices are strategic complements (whereas quantities are strategic substitutes), transfer prices above marginal cost come with the silver lining of softening competitors' responses (Bulow et al. 1985). The end result is that the competitive cost of decentralization is less pronounced under price competition than quantity competition, making decentralization even more attractive under price competition.

The fact that upstream pricing drives the benefit of decentralization in this setup, coupled with the fact that such benefits are undercut by competitive concerns, is stressed in the next corollary, which presents the simple case of downstream duopoly. In particular, the corollary highlights that the essential results are invariant to the form of competition.

Corollary 2: Under duopoly ($n = 1$),

(i) Decentralization is preferred to centralization under both price and quantity competition.
(ii) Under either form of competition, the net benefit of decentralization is decreasing in k.

Not only are the results herein robust to the form of competition, but, as the next section reveals, the key forces persist even when the downstream division can influence the chosen transfer price. To demonstrate this result, we return to our base setup of quantity competition, and maintain this assumption throughout the remainder of the paper.

Generalized Nash Bargaining

In the basic setup, transfer prices are assumed to be set unilaterally by the upstream division under decentralization. While such a case is often used to capture standard-cost transfer pricing with no restrictions on reported costs, other scenarios warrant a more general understanding of the relative influence of the two divisions on the agreed-upon price. An example is negotiated transfer pricing in which the two divisions jointly reach agreement on the appropriate transfer price.

This section seeks to generalize the relative bargaining power of the two divisions in setting transfer prices under decentralization. In doing so, we employ the standard Nash bargaining solution, generalized to (possibly) asymmetric bargaining power (e.g., Myerson 1991).[10] In particular, the chosen transfer price under decentralization and quantity competition is the outcome of a bargaining process that solves the generalized Nash product of the two divisions' profits:

$$\underset{t}{Max}\ [q_0^D(p,t)(t - c_I)]^{\beta}[q_0^D(p,t)(a - q_0^D(p,t) - \sum_{j \neq 0} kq_j^D(p,t)) - q_0^D(p,t)(p + t)]^{1-\beta}. \tag{16}$$

In Equation (16), $\beta \in (0,1]$ reflects the relative bargaining power of the upstream division ($\beta = 1$ corresponds to the base setting in which the upstream division has complete control).[11] Jointly solving the first-order conditions of Equations (5) and (16) reveals $p^D(k,n,\beta)$ and $t^D(k,n,\beta)$, the equilibrium pricing of the supplier and the decentralized firm, respectively. Substituting these in $q_i^D(p,t)$ yields equilibrium production levels; plugging these production levels in Equations (1) and (2) yields profits for the firms, $\Pi_i^D(k,n,\beta)$. Comparing $\Pi_0^D(k,n,\beta)$ and $\Pi_0^C(k,n)$ yields Proposition 3.

Proposition 3: Under generalized Nash bargaining between divisions,

(i) If $n \leq 2$, then decentralization yields higher firm profit.
(ii) If $n > 2$, then decentralization yields higher firm profit if and only if $k < k^*(n,\beta)$, where:

$$k^*(n,\beta) = \frac{((n-1)(8-5\beta) + \sqrt{[8-5\beta][(8-5\beta)(n-1)^2 + 32(2-\beta)n]})}{8(2-\beta)n}.$$

Proposition 3 serves to generalize the results in Proposition 1 to varying degrees of bargaining power in setting transfer prices. As should be expected, $k^*(n,1) = k^*(n)$, i.e., the previously analyzed scenario amounts to a special case of the above result. Also, some tedious algebra confirms that $k^*(n,\beta) \geq k^*(n)$. This implies that if the conditions of Proposition 1 are satisfied, then the results hold for any β. In other words, the result that decentralization can be beneficial because it serves to cut supplier pricing terms (as outlined in Proposition 1) is robust to the particular form of bargaining that gives rise to the prevailing transfer price—what is important is that transfer pricing is decentralized, not the means through which such prices are chosen.

A natural question that arises in this case regards the firm's preferred bargaining arrangement. That is, if the firm could determine the relative influence and decision-making power of the divisions when establishing its decentralized structure, then what would be the optimal way of doling out such power? The next proposition answers this question.

Proposition 4: (i) The firm's preferred bargaining arrangement is $\beta = \beta^*(k,n)$, where:

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[10] Besides allowing an axiomatic representation of bargaining outcomes, this approach is commonly employed because it also allows a tractable characterization of equilibria without an explicit representation of the specific bargaining process.

[11] By excluding $\beta = 0$, we focus on cases where there is at least some party in negotiations that presses for setting transfer prices at more than marginal cost.

$$\beta^*(k,n) = \frac{8 + 4k(n-1) - 4k^2n}{8 + 4k(n-1) - 3k^2n}.$$

(ii) Decentralization with $\beta = \beta^*(k,n)$ replicates the firm's optimal transfer-pricing policy.

Part (i) of the proposition reveals the β that maximizes $\Pi_0^D(k,n,\beta)$. Part (ii) notes that this arrangement has an intriguing property. It not only represents the firm's preferred bargaining arrangement, but it also replicates the transfer price the firm would have chosen had it been able to precommit to a transfer price. More specifically, say the firm could preempt any supplier price (acting as a first mover) by announcing an irrevocable transfer price. This would entail the supplier solving Equation (5) for the announced t. Substituting the resulting supplier price, denoted $p^D(t)$, in $q_i^D(p,t)$ enables firm profit to be expressed as a function of t. In this case, the firm solves:

$$\underset{t}{Max}\; q_0^D(p^D(t),t)[a - q_0^D(p^D(t),t) - \sum_{j\neq 0} kq_j^D(p^D(t),t)] - q_0^D(p^D(t),t)[p^D(t) + c_I]. \quad (17)$$

Denoting the solution to Equation (17) by $t^*(k,n)$, Proposition 4 (ii) states that $t^D(k,n,\beta^*(k,n)) = t^*(k,n)$, and, hence, firm value under decentralization is equivalent to that in the case of a precommitted transfer price by the firm. This suggests a return to the issue of commitment. While the firm's *ex post* temptation to adjust transfer prices toward marginal cost may make a centrally established transfer price difficult to credibly enforce, a commitment to a specific organizational structure may be more credible. And, given the organizational structure, the chosen transfer price is self-enforcing. As the proposition shows, if decision rights are judiciously decentralized, then such an approach entails no loss in firm profit.

Roughly stated, $\beta^*(k,n)$ reflects the extent to which the firm wants to hand the reins of control to the upstream division. Note that $\beta^*(k,0) = 1$, indicating that in the case of downstream monopoly, the simple rule where the upstream division unilaterally sets prices is optimal. Competition introduces costs of such an approach, however, since the upstream division does not internalize the entire competitive loss that comes from transfer prices above marginal cost. Consistent with this view, the firm's preferred arrangement shifts bargaining power to the downstream division as competitive pressures become more pronounced, i.e., $\beta^*(k,n)$ is decreasing in k and n.

IV. VARIATIONS IN THE SUPPLY MARKET

Multiple Input Suppliers

Thus far, the analysis has honed in on a case of monopoly supply of inputs to underscore the notion that decentralization and the ensuing transfer-pricing issues can serve to convince a supplier to cut its price. In this section, we expand the analysis to consider the effect of upstream price competition on the attractiveness of decentralization. In particular, consider the case of multiple suppliers of the external good. Without loss of generality, denote the production cost of the most efficient producer of the input by c_E and the production cost of its nearest competitor by $c_E + \Delta$, $\Delta \geq 0$ (the production costs of any remaining suppliers will, of course, not affect equilibrium outcomes under price competition). In effect, Δ captures the intensity of upstream price competition: $\Delta = 0$ corresponds to perfect competition, whereas sufficiently large Δ is indicative of upstream monopoly (since the least-cost supplier's rivals are unable to undercut its pricing).

The next lemma presents the equilibrium profits of firm 0 in this case under centralization and decentralization, respectively. In equilibrium, the input is supplied by the most efficient (least-cost) producer, and it is provided at a price that is no higher than the cost of production of its closest competitor.

Lemma 3: With multiple input suppliers, the equilibrium profits for firm 0 under centralization ($\Pi_0^C(\Delta,k,n)$) and decentralization ($\Pi_0^D(\Delta,k,n)$) equal:

$$\begin{aligned}
\Pi_0^C(\Delta,k,n) &= \Pi_0^C(k,n)\left[2 - \frac{\Delta}{\Delta_H}\right]^2 && \text{if } \Delta \leq \Delta_H = \frac{(a - c_E - c_I)(2 - k)}{2[2 + k(n - 1)]}, \\
&= \Pi_0^C(k,n) && \text{if } \Delta > \Delta_H. \\
\Pi_0^D(\Delta,k,n) &= \Pi_0^D(k,n)\left[\frac{3}{2} - \frac{\Delta}{2\Delta_L}\right]^2 && \text{if } \Delta \leq \Delta_L = \frac{(a - c_E - c_I)(2 - k)}{3[2 + k(n - 1)]}, \\
&= \Pi_0^D(k,n) && \text{if } \Delta > \Delta_L.
\end{aligned}$$

The lemma confirms that for sufficiently small Δ-values (i.e., enough intensity of upstream competition), firm 0's profit is higher because competition lowers the equilibrium price of the external good. Intuitively, then, upstream competition may substitute for decentralization as a means of cutting supplier prices. This translates into decentralization being most attractive when the input supply market is characterized by weak competition, as the following corollary confirms.

Corollary 3: Less competition in the supply market increases the attractiveness of decentralization. That is, $\frac{d[\Pi_0^D(\Delta,k,n) - \Pi_0^C(\Delta,k,n)]}{d\Delta} \geq 0$, where the inequality is strict for $\Delta < \Delta_H$.

The intuition from Corollary 3 carries forward to the paper's main result. In particular, though upstream competition may itself reduce input prices, it does not preclude decentralization as a preferred organizational form unless it is sufficiently intense.

Proposition 5: With multiple input suppliers:

(i) If $n \leq 2$, then decentralization yields higher firm profit if and only if $\Delta > \Delta^*(k,n)$.

(ii) If $n > 2$, then decentralization yields higher firm profit if and only if $k < k^*(n)$ and $\Delta > \Delta^*(k,n)$, where:

$$\Delta^*(k,n) = \frac{(a - c_E - c_I)(2 - k)}{2 + k(n - 1)}\left[1 - \frac{1}{3}\sqrt{\frac{6 + 3k(n - 1) - k^2n}{2 + k(n - 1)}}\right].$$

The difference between Propositions 1 and 5 stems solely from the addition of the Δ-condition. To get a feel for the condition, first note that when $k < k^*(n)$, $\Delta_L < \Delta^*(k,n) < \Delta_H$. From Lemma 3, if $\Delta > \Delta_H$, then the outcomes are precisely as in Section III; in this case, the result in Proposition 5 is also equivalent to that in Proposition 1. For such large Δ-values, upstream competition is so weak that competitors have no effect. At the other extreme, if $\Delta < \Delta_L$ the supply market is so competitive that organizational structure has no effect on pricing. In this case, centralization always wins out, as also confirmed by Proposition 5.

For intermediate Δ-values, competition in the supply market undercuts supplier pricing under centralization. Nonetheless, provided upstream and downstream competition are not too intense (i.e., $\Delta > \Delta^*(k,n)$ and $k < k^*(n)$), firm 0 finds it worthwhile to decentralize so as to push down supplier prices even further. Thus, besides demonstrating that the key forces of the setting can persist under modest upstream competition, the result also suggests that, all else equal, one would expect to see decentralization combat excessive upstream pricing in markets that are characterized by less intense competition at all levels of the supply chain. The next subsection considers another variation in the supply market.

Multiple Input Buyers

We have thus far considered the organizational structure choice of one firm that relies on an external supplier for an input. The firm's competitors, on the other hand, are presumed not to need the supplier for inputs. This simplifying assumption allows a focus on the upstream pricing consequences of one firm's organizational structure. The results suggest that upstream pricing considerations may undergird a unilateral incentive to decentralize and tolerate the ensuing transfer-pricing distortions. The question remains, however, if such an incentive persists when competitors too rely on external supply.

To address the above question, consider a duopoly downstream market (i.e., $n = 1$) wherein each firm produces one input in-house and relies on a (sole) supplier for its second input. In this case, the firms first (simultaneously) choose whether to decentralize, after which the game proceeds as before: input prices are set, downstream quantities are chosen, and consumer demand is realized. In this symmetric procurement setting, we next show that the essential tensions identified in previous sections lead to decentralization by both firms being the unique Nash equilibrium. As before, we employ backward induction to identify relevant equilibria.

Outcome If Both Firms Are Centralized

Given supplier price p_i for firm i, $i = 0,1$, firm i chooses q_i to maximize its profit in the final product market. That is, firm i solves:

$$\underset{q_i}{Max}\; q_i[a - q_i - kq_j] - q_i[p_i + c_I],\; i,j = 0,1; i \neq j. \tag{18}$$

Jointly solving the two first-order conditions associated with Equation (18) yields equilibrium quantities as a function of the supplier price, denoted $q_i^{CC}(p_i,p_j)$, where the first superscript indicates that the subscripted firm is centralized, and the second superscript indicates its rival is centralized as well. Given these quantities, the supplier sets prices to solve:

$$\underset{p_0,p_1}{Max}\; q_0^{CC}(p_0,p_1)[p_0 - c_E] + q_1^{CC}(p_1,p_0)[p_1 - c_E]. \tag{19}$$

The first-order condition of Equation (19) yields $p_i^{CC}(k)$, the optimal prices set by the supplier for the external good when it supplies to two centralized firms. Substituting $p_i^{CC}(k)$ in $q_i^{CC}(p_i,p_j)$ yields $q_i^{CC}(k)$, firm i's production level. Substituting these in Equation (18) yields firm i's profit, denoted $\Pi_i^{CC}(k)$. The results of these calculations are presented in Equation (20).

$$p_i^{CC}(k) = c_E + \frac{a - c_E - c_I}{2};\ q_i^{CC}(k) = \frac{a - c_E - c_I}{2(2 + k)};\ \text{and}$$
$$\Pi_i^{CC}(k) = \frac{(a - c_E - c_I)^2}{4(2 + k)^2}. \tag{20}$$

Outcome If Firm i Is Centralized and Firm j Is Decentralized

Given supplier price p_i, the centralized firm i chooses q_i to maximize its profit:

$$\underset{q_i}{Max}\ q_i[a - q_i - kq_j] - q_i[p_i + c_I]. \tag{21}$$

The downstream division of the decentralized firm, firm j, solves:

$$\underset{q_j}{Max}\ q_j[a - q_j - kq_i] - q_j[p_j + t_j]. \tag{22}$$

Solving the first-order conditions of Equations (21) and (22) yields equilibrium quantities as a function of the input prices, denoted $q_i^{CD}(p_i,p_j,t_j)$ and $q_j^{DC}(p_j,p_i,t_j)$, where the superscript D indicates firm j is decentralized. Given these quantities, the supplier chooses p_i and p_j, and the upstream division of firm j simultaneously chooses t_j, to maximize their respective profits. The supplier's problem is in Equation (23), and the upstream division's problem is in Equation (24):

$$\underset{p_i,p_j}{Max}\ q_i^{CD}(p_i,p_j,t_j)[p_i - c_E] + q_j^{DC}(p_j,p_i,t_j)[p_j - c_E]. \tag{23}$$

$$\underset{t_j}{Max}\ q_j^{DC}(p_j,p_i,t_j)[t_j - c_I]. \tag{24}$$

Jointly solving the first-order conditions of Equations (23) and (24) yields $p_i^{CD}(k)$, the price paid by firm i for the external good, and $p_j^{DC}(k)$ and $t_j^{DC}(k)$, the price paid by the downstream division of firm j for its two inputs. Substituting these prices in $q_i^{CD}(p_i,p_j,t_j)$ and $q_j^{DC}(p_j,p_i,t_j)$ yields $q_i^{CD}(k)$ and $q_j^{DC}(k)$, the production levels of the centralized and the decentralized firm, respectively. Substituting these in Equation (18) yields profits for the two firms, denoted $\Pi_i^{CD}(k)$ and $\Pi_j^{DC}(k)$. These expressions are presented in Equation (25):

$$p_i^{CD}(k) = c_E + \frac{a - c_E - c_I}{2};\ q_i^{CD}(k) = \frac{(a - c_E - c_I)(6 + k)}{12(2 + k)};$$
$$\Pi_i^{CD}(k) = \frac{(a - c_E - c_I)^2(6 + k)^2}{144(2 + k)^2};\ p_j^{DC}(k) = c_E + \frac{(a - c_E - c_I)(4 + k)}{12};$$
$$t_j^{DC}(k) = c_I + \frac{(a - c_E - c_I)(2 - k)}{6};\ q_j^{DC}(k) = \frac{a - c_E - c_I}{3(2 + k)};$$
$$\Pi_j^{DC}(k) = \frac{(a - c_E - c_I)^2(6 - k^2)}{18(2 + k)^2}. \tag{25}$$

Outcome If Both Firms Are Decentralized

Given the supplier charges p_i for the external good and firm i's upstream division charges t_i for the internal good, the downstream division of firm i chooses q_i to maximize:

$$\underset{q_i}{Max}\ q_i[a - q_i - kq_j] - q_i[p_i + t_i],\ i,j = 0,1; i \neq j. \tag{26}$$

Solving the two first-order conditions associated with Equation (26) yields equilibrium quantities as a function of the input prices, denoted $q_i^{DD}(p_i,p_j,t_i,t_j)$. Given these quantities, the supplier chooses p_i and p_j, and the upstream divisions of firm i and firm j choose t_i and t_j, respectively, to maximize their own profits. The supplier's problem is in Equation (27), and each upstream division's problem is in Equation (28):

$$\underset{p_0,p_1}{Max}\ q_0^{DD}(p_0,p_1,t_0,t_1)[p_0 - c_E] + q_1^{DD}(p_1,p_0,t_1,t_0)[p_1 - c_E]. \tag{27}$$

$$\underset{t_i}{Max}\ q_i^{DD}(p_i,p_j,t_i,t_j)[t_i - c_I],\ i,j = 0,1; i \neq j. \tag{28}$$

Jointly solving the first-order conditions of Equations (27) and (28) yields $p_i^{DD}(k)$ and $t_i^{DD}(k)$, the price paid by the downstream division of firm i for each of its inputs. Substituting these prices into $q_i^{DD}(p_i,p_j,t_i,t_j)$ yields $q_i^{DD}(k)$, firm i's production level. Substituting these in Equation (18) yields firm i's profit, denoted $\Pi_i^{DD}(k)$. These calculations are in Equation (29):

$$p_i^{DD}(k) = c_E + \frac{2(a - c_E - c_I)}{6 - k};\ t_i^{DD}(k) = c_I + \frac{(a - c_E - c_I)(2 - k)}{6 - k};$$
$$q_i^{DD}(k) = \frac{2(a - c_E - c_I)}{(2 + k)(6 - k)};\ \Pi_i^{DD}(k) = \frac{(a - c_E - c_I)^2(12 - 2k^2)}{(2 + k)^2(6 - k)^2}. \tag{29}$$

Given the outcomes for each firm's centralization/decentralization choice, we next identify the conditions under which decentralization is an equilibrium outcome. Using the profit expressions in Equations (20), (25), and (29), Table 1 presents the payoffs in the firms' 2 × 2 game.

The next proposition follows directly from a comparison of terms in Table 1.

Proposition 6: In the duopoly case when both firms rely on the supplier, decentralization is a dominant choice for each firm.

Proposition 6 shows that firm 0's unilateral preference for decentralization is not derailed when its rival is also dependent on the same supplier for a critical input and must make a symmetric choice As before, the reasoning is that decentralization enables a firm to obtain more favorable supplier terms. In fact, if firm i is centralized, then it obtains the external good at the price $c_E + \frac{a - c_E - c_I}{2}$ independent of firm j's organizational structure. However, the price is lowered to $p_i^{DC}(k)$ if firm i is the only decentralized firm, and the price is lowered still further to $p_i^{DD}(k)$ if both firms are decentralized.[12]

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[12] When both firms are decentralized, they are on equal competitive footing, thereby intensifying duopoly competition; this, in turn, makes them even more sensitive to supplier pricing. In such cases, the supplier responds by further lowering its price. As a result, both firms are better off with the joint decision to decentralize than they would have been under any other combination of organizational structures. This is in contrast to extant models of strategic precommitments (e.g., Allaz and Vila 1993; Hughes and Williams 2006) wherein firms typically find themselves worse off by the ability to make strategic precommitments when a rival also has such capabilities.

TABLE 1
Payoff Matrix for the Duopoly Case

	Centralize	*Decentralize*
Centralize	$\frac{(a-c_E-c_I)^2}{4(2+k)^2}$ $\frac{(a-c_E-c_I)^2}{4(2+k)^2}$	$\frac{(a-c_E-c_I)^2(6+k)^2}{144(2+k)^2}$ $\frac{(a-c_E-c_I)^2(6-k^2)}{18(2+k)^2}$
Decentralize	$\frac{(a-c_E-c_I)^2(6-k^2)}{18(2+k)^2}$ $\frac{(a-c_E-c_I)^2(6+k)^2}{144(2+k)^2}$	$\frac{(a-c_E-c_I)^2(12-2k^2)}{(2+k)^2(6-k)^2}$ $\frac{(a-c_E-c_I)^2(12-2k^2)}{(2+k)^2(6-k)^2}$

Rows (columns) indicate firm 0's (1's) organizational structure.
The upper left (lower right) value in each cell is firm 0's (1's) profit.

In the symmetric firms setup, firm 0 faces a weaker rival, one that also procures the external good at more than marginal cost. This suggests that firm 0's incentives to decentralize might actually be amplified in the symmetric setup, since the downside of lower competitiveness is mitigated by the fact that a competitor too may decentralize. The next proposition addresses this issue in the n-rival case, and shows that this intuition is indeed borne out. In particular, the condition under which decentralization by the firms is a Nash equilibrium when all firms rely on the external supplier is (at least weakly) more relaxed than the condition in Proposition 1.

Proposition 7: When all firms rely on the supplier,

(i) If $n \leq 5$, then decentralization by (all of) the firms is a Nash equilibrium.
(ii) If $n > 5$, then decentralization by the firms is a Nash equilibrium if and only if $k < \bar{k}(n)$, where $\bar{k}(n) > k^*(n)$.

Taken together, Propositions 5, 6, and 7 speak to the robustness of decentralization as a means of influencing supplier pricing. That is, whether there are many suppliers or many customers of an input, decentralization by a firm can serve to curtail supplier pricing. This feature, if sufficiently pronounced relative to the detrimental consequences for downstream competitiveness, can justify decentralization as the preferred organizational structure.

V. CONCLUSION

This paper shows that a firm wishing to obtain better terms from a supplier may be willing to tolerate potential transfer-pricing conflicts and delegate pertinent decisions to division managers. The model presents a scenario that, at first glance, puts decentralization and transfer-pricing policy in a bad light. Excessive internal transfer prices only cut into production that is already depressed by excessive supplier pricing. The presence of an external supplier, however, introduces a delicate interaction: distortions in the intra-firm supply chain impact the pricing along the inter-firm supply chain. Forced to pay more than marginal cost even for the internal good, a downstream division exhibits dampened enthusiasm to produce, which, in turn, seeps over to the supplier's pricing. Recognizing that the procuring party is increasingly wary of high prices, the supplier's best response is to curtail price markups so as to induce greater demand. From the firm's perspective, decentralization introduces competing tensions: there is a cost stemming from production distortions brought by internal price markups, but also a benefit of reining in external supplier prices. The paper formalizes conditions under which the benefit of curtailing supplier aggressiveness makes decentralization worthwhile for the firm.

In this light, the contention that decentralization may be beneficial as a means of cutting supplier prices seems most appropriate in circumstances in which substantial frictions exist in the supply market. In such situations, other remedies may also be available. Most notably, an attempt to vertically integrate can remove supplier pricing concerns and, thus, eliminate the need to decentralize. However, regulatory considerations may preclude such mergers, particularly when competitors too rely on the supplier. Even when vertical mergers are feasible, our analysis may provide insights about the relative bargaining leverage of the merging parties. In particular, the paper stresses that in the absence of an agreement to integrate, the downstream firm's preferred option can be to further fractionalize the channel; though such a move benefits the firm, it substantially reduces supplier profits. With such a disagreement point looming, the supplier may be more willing to make concessions in negotiation.[13]

While the paper focuses attention on a standard decentralized arrangement wherein each division focuses on its own divisional profit, another consideration is whether the firm can benefit by refining compensation structures to better exploit decentralization. For example, the downstream division could be organized as a cost center, with the division manager being rewarded only if the external good is procured at a price below a certain threshold. In this case, the supplier will be reluctant to set prices above that threshold knowing that the division will simply stop procuring the good. The downside of such an approach is that the downstream division's fixation on costs may distort its incentives to procure the appropriate quantity and/or quality of inputs.

The paper's results may also have implications for the assignment of ownership rights. The benefits achieved herein by decentralizing decisions can also be achieved by decentralizing ownership, say by choosing to spin off a division. If a firm opts to spin off its upstream operations, it creates additional upstream friction in the supply channel. Knowing such friction will reduce the other external supplier's price, the firm can garner greater value from the spin-off; spin-off proceeds plus the expected value of the retained unit can exceed the value of a (centralized) conglomerate.

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[13] Another issue arises if the supplier can charge a two-part tariff, in which case it could eliminate frictions by charging marginal cost and extracting surplus via a fixed "access" fee. In such cases, if the upstream division can also charge a fixed fee, it can use its fixed charge to extract some of the surplus away from the supplier and bring it back to the firm. As long as the resulting equilibrium entails the upstream division obtaining a nonzero share of the surplus (which is the case, for example, if weakly dominated strategies are ruled out), a benefit of decentralization again arises. Here, the relaxation of supplier pricing comes in the form of a cut in its fixed fee.

APPENDIX

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Proof of Lemma 1

Under centralization, firm 0 chooses q_0 to maximize Equation (1). The firm's (Cournot) rivals choose q_i, $i \neq 0$, to maximize Equation (2). Solving the maximization problems jointly yields $q_i^C(p)$, $i = 0,\ldots,n$, the equilibrium quantities as a function of supplier price:

$$q_0^C(p) = \frac{(a - p - c_I)(2 - k) - kn(p - c_E)}{(2 - k)(2 + kn)}. \tag{A1}$$

$$q_i^C(p) = \frac{(a - c_E - c_I)(2 - k) + k(p - c_E)}{(2 - k)(2 + kn)}, \; i \neq 0. \tag{A2}$$

Using $q_0^C(p)$ from (A1), the supplier sets price p to maximize its profits in Equation (3). The first-order condition of Equation (3) with respect to p yields the $p^C(k,n)$ in Lemma 1. Substituting $p^C(k,n)$ in (A1) and (A2) yields $q_0^C(k,n)$ and $q_i^C(k,n)$, $i \neq 0$, respectively. Finally, substituting $q_0^C(k,n)$ and $q_i^C(k,n)$ in Equation (1) and Equation (2) yields $\Pi_0^C(k,n)$ and $\Pi_i^C(k,n)$, $i \neq 0$, respectively. This completes the proof of Lemma 1. ■

Proof of Lemma 2

Under decentralization, the downstream division of firm 0 chooses q_0 to maximize Equation (4). The firm's rivals choose q_i, $i \neq 0$, to maximize Equation (2). Solving the maximization problems jointly yields $q_i^D(p,t)$, $i = 0,\ldots,n$, the equilibrium quantities as a function of supplier price and transfer price:

$$q_0^D(p,t) = \frac{(a - p - t)(2 - k) - kn(p - c_E + t - c_I)}{(2 - k)(2 + kn)}. \tag{A3}$$

$$q_i^D(p,t) = \frac{(a - c_E - c_I)(2 - k) + k(p - c_E + t - c_I)}{(2 - k)(2 + kn)}, \; i \neq 0. \tag{A4}$$

Using $q_0^D(p,t)$ from (A3), the supplier's problem is in Equation (5), and the upstream division's problem is in Equation (6). Jointly solving the first-order conditions of Equation (5) and Equation (6) with respect to p and t, respectively, yields $p^D(k,n)$ and $t^D(k,n)$ in Lemma 2. Substituting $p^D(k,n)$ and $t^D(k,n)$ in (A3) and (A4) yields $q_0^D(k,n)$ and $q_i^D(k,n)$, $i \neq 0$, respectively. Finally, substituting $q_0^D(k,n)$ and $q_i^D(k,n)$ in Equation (1) and Equation (2) yields $\Pi_0^D(k,n)$ and $\Pi_i^D(k,n)$, $i \neq 0$, respectively. This completes the proof of Lemma 2. ■

Proof of Proposition 1

From Lemma 1 and Lemma 2:

$$\Pi_0^D(k,n) - \Pi_0^C(k,n) = \frac{(a - c_E - c_I)^2[6 + 3k(n - 1) - 4k^2n]}{36[2 + k(n - 1)][2 + kn]^2}. \tag{A5}$$

(i) Substituting $n = 0$, $n = 1$, and $n = 2$ in (A5) yields:

$$\Pi_0^D(k,0) - \Pi_0^C(k,0) = \frac{(a - c_E - c_I)^2}{48} > 0.$$

$$\Pi_0^D(k,1) - \Pi_0^C(k,1) = \frac{(a - c_E - c_I)^2(3 - 2k^2)}{36(2 + k)^2} > 0.$$

$$\Pi_0^D(k,2) - \Pi_0^C(k,2) = \frac{(a - c_E - c_I)^2[6(1 - k^2) + k(3 - 2k)]}{144(2 + k)(1 + k)^2} > 0.$$

(ii) The difference in (A5) is positive if and only if $6 + 3k(n - 1) - 4k^2n > 0$. The LHS is a quadratic concave function in k, and its two roots are $\frac{3(n - 1) \pm \sqrt{9 + 78n + 9n^2}}{8n}$. The smaller of the two roots is negative. The larger, positive root is denoted by $k^*(n)$ in the proposition. Note $0 < k^*(n) \leq 1$ for $n \geq 3$. Given concavity of the function, for $k \in [0,k^*(n))$, $6 + 3k(n - 1) - 4k^2n > 0$ and, hence, decentralization is (strictly) preferred. On the other hand, for $k \in (k^*(n),1]$ centralization is preferred. ■

Proof of Corollary 1

(i) The derivative of $k^*(n)$ with respect to n is:

$$\frac{dk^*(n)}{dn} = -\frac{\sqrt{3}[13n + 3 - \sqrt{3(3 + 26n + 3n^2)}]}{8n^2\sqrt{3 + 26n + 3n^2}} < 0.$$

(ii) Using L'Hospital's rule, the limit of $k^*(n)$ as $n \rightarrow \infty$ is 3/4. ■

Proof of Proposition 2

Under centralization, firm 0 chooses P_0 to maximize Equation (10). The firm's (Bertrand) rivals choose P_i, $i \neq 0$, to maximize Equation (11). Solving the maximization problems jointly yields $P_i^C(p)$, $i = 0,\ldots,n$, the equilibrium product price as a function of supplier input price:

$$P_0^C(p) = \frac{[a + p + c_I]A + (p + c_I)B - (p - c_E)C}{4 + 6k(n - 1) + k^2(2 - 5n + 2n^2)}. \tag{A6}$$

$$P_i^C(p) = \frac{[a + c_E + c_I]A + (c_E + c_I)B + (p - c_E)D}{4 + 6k(n - 1) + k^2(2 - 5n + 2n^2)}, \tag{A7}$$

where:

$A = [1 - k][2 + k(2n - 1)]$;
$B = kn[2 + k(2n - 1)]$;
$C = kn[1 + k(n - 1)]$; and
$D = k[1 + k(n - 1)]$.

Substituting (A6) and (A7) in the demand functions in Equation (9) yields $q_0^C(P_0^C(p), P_i^C(p))$ and $q_i^C(P_0^C(p), P_i^C(p))$, $i \neq 0$, the equilibrium quantities as a function of supplier price.

The supplier sets price p to maximize its profit in Equation (12). The first-order condition of Equation (12) with respect to p yields $\tilde{p}^C(k,n)$, the equilibrium supplier price:

$$\tilde{p}^C(k,n) = c_E + \frac{[a - c_E - c_I][1 - k][2 + k(2n - 1)]}{2[2 + 3k(n - 1) + k^2(1 - 3n + n^2)]}. \tag{A8}$$

Substituting (A8) in (A6) and (A7) yields equilibrium product prices, and using these prices in Equation (10) yields the following equilibrium profit for firm 0:

$$\tilde{\Pi}_0^C(k,n) = \frac{(a - c_E - c_I)^2[1 - k][1 + k(n - 1)]}{4[2 + k(n - 2)]^2[1 + kn]}. \tag{A9}$$

Under decentralization, the downstream division of firm 0 chooses P_0 to maximize Equation (13). The firm's rivals choose P_i, $i \neq 0$, to maximize Equation (11). Solving the maximization problems jointly yields $P_i^D(p,t)$, $i = 0,\ldots,n$, the equilibrium prices as a function of supplier price and transfer price:

$$P_0^D(p,t) = \frac{[a + p + t]A + (p + t)B - (p - c_E)C - (t - c_I)C}{4 + 6k(n - 1) + k^2(2 - 5n + 2n^2)}. \tag{A10}$$

$$P_i^D(p,t) = \frac{[a + c_E + t]A + (c_E + t)B + (p - c_E)D - (t - c_I)E}{4 + 6k(n - 1) + k^2(2 - 5n + 2n^2)}, \tag{A11}$$

where:

$$E = 2[1 + k(n - 1)]^2.$$

Using $q_0(\cdot)$ from Equation (9) and $P_0^D(p,t)$ and $P_i^D(p,t)$ from (A10) and (A11), the supplier's problem is in Equation (14), and the upstream division's problem is in Equation (15). Solving the first-order conditions of Equations (14) and (15) with respect to p and t, respectively, yields $\tilde{p}^D(k,n)$ and $\tilde{t}^D(k,n)$:

$$\tilde{p}^D(k,n) = c_E + \frac{[a - c_E - c_I][1 - k][2 + k(2n - 1)]}{3[2 + 3k(n - 1) + k^2(1 - 3n + n^2)]}. \tag{A12}$$

$$\tilde{t}^D(k,n) = c_I + \frac{[a - c_E - c_I][1 - k][2 + k(2n - 1)]}{3[2 + 3k(n - 1) + k^2(1 - 3n + n^2)]}. \tag{A13}$$

Substituting $\tilde{p}^D(k,n)$and $\tilde{t}^D(k,n)$ into (A10), (A11), and Equation (9) yields equilibrium prices and quantities. Using these in Equation (1) yields the following equilibrium profit for firm 0:

$$\tilde{\Pi}_0^D(k,n) = \frac{\begin{array}{l}(a - c_E - c_I)^2[1 - k][6 \\ \quad + 15k(n - 1) + 2k^2(6 - 13n + 6n^2) + k^3(3(n^3 - 1) - 11n(n - 1))]\end{array}}{9[2 + k(n - 2)]^2[2 + k(5n - 3) + k^3n(1 - 3n + n^2) + k^2(1 - 6n + 4n^2)]}. \tag{A14}$$

Using (A9) and (A14), the difference in firm 0's profit under decentralization and centralization is:

$$\tilde{\Pi}_0^D(k,n) - \tilde{\Pi}_0^C(k,n) = \frac{\begin{array}{l}(a - c_E - c_I)^2[1 - k][6 + 15k(n - 1) + k^2(12 - 23n + 12n^2)\\ \quad + k^3(3(n^3 - 1) - 8n(n - 1))]\end{array}}{36[2 + k(n - 2)]^2[2 + k(5n - 3) + k^3n(1 - 3n + n^2) + k^2(1 - 6n + 4n^2)]}. \quad (A15)$$

Finally, tedious algebra confirms (A15) is positive for all $n \geq 0$ and $0 \leq k < 1$. This completes the proof of Proposition 2. ■

Proof of Corollary 2

(i) Using (A5) and (A15):

$$\Pi_0^D(k,1) - \Pi_0^C(k,1) = \frac{(a - c_E - c_I)^2(3 - 2k^2)}{36(2 + k)^2} > 0.$$

$$\tilde{\Pi}_0^D(k,1) - \tilde{\Pi}_0^C(k,1) = \frac{(a - c_E - c_I)^2[1 - k][6 + k^2]}{36[2 - k]^2[1 + k][2 - k^2]} > 0.$$

(ii) The derivative of each of the above expressions with respect to k is given below, and the sign is assigned using the fact that k is bounded by 0 and 1:

$$\frac{d[\Pi_0^D(k,1) - \Pi_0^C(k,1)]}{dk} = -\frac{(a - c_E - c_I)^2[3 + 4k]}{18[2 + k]^3} < 0.$$

$$\frac{d[\tilde{\Pi}_0^D(k,1) - \tilde{\Pi}_0^C(k,1)]}{dk} = -\frac{(a - c_E - c_I)^2[12 - 28k + 16k^2 + 20k^3 - 13k^4 + k^5 - k^6]}{18[2 - k]^3[1 + k]^2[2 - k^2]^2} < 0. \quad ■$$

Proof of Proposition 3

Under decentralization and Nash bargaining, the chosen t maximizes the following Nash product, wherein $q_0^D(p,t)$ and $q_i^D(p,t)$, $i \neq 0$, are as in (A3) and (A4):

$$\max_t \, [q_0^D(p,t)(t - c_I)]^{\beta}[q_0^D(p,t)(a - q_0^D(p,t) - nkq_i^D(p,t)) - q_0^D(p,t)(p + t)]^{-\beta}. \quad (A16)$$

Using $q_0^D(p,t)$ from (A3), the supplier's problem is in Equation (5). Jointly solving the first-order conditions of Equation (5) and (A16) with respect to p and t, respectively, yields $p^D(k,n,\beta)$ and $t^D(k,n,\beta)$:

$$p^D(k,n,\beta) = c_E + \frac{(a - c_E - c_I)[2 - k][2 - \beta]}{[2 + k(n - 1)][4 - \beta]}. \quad (A17)$$

$$t^D(k,n,\beta) = c_I + \frac{(a - c_E - c_I)[2 - k]\beta}{[2 + k(n - 1)][4 - \beta]}. \quad (A18)$$

Substituting $p^D(k,n,\beta)$ and $t^D(k,n,\beta)$ in (A3) and (A4) yields $q_0^D(k,n,\beta)$ and $q_i^D(k,n,\beta)$, $i \neq 0$, respectively. Finally, substituting $q_0^D(k,n,\beta)$ and $q_i^D(k,n,\beta)$ in Equation (1) yields $\Pi_0^D(k,n,\beta)$:

$$\Pi_0^D(k,n,\beta) = \frac{(a - c_E - c_I)^2[(2 + \beta)(2 + k(n - 1)) - \beta k^2 n][2 - \beta]}{[2 + k(n - 1)][2 + kn]^2[4 - \beta]^2}. \tag{A19}$$

Using $\Pi_0^C(k,n)$ from Lemma 1 and $\Pi_0^D(k,n,\beta)$ from (A19) yields:

$$\Pi_0^D(k,n,\beta) - \Pi_0^C(k,n) = \frac{(a - c_E - c_I)^2[(8 - 5\beta)(2 + k(n - 1)) - 4k^2 n(2 - \beta)]\beta}{4[2 + k(n - 1)][2 + kn]^2[4 - \beta]^2}. \tag{A20}$$

It is easy to verify that (A20) is positive for $n = 0$, 1, and 2. Turning to part (ii), the difference in (A20) is positive if and only if $(8 - 5\beta)(2 + k(n - 1)) - 4k^2n(2 - \beta) > 0$. The LHS is a quadratic concave function in k, with only one positive root. This root is denoted by $k^*(n,\beta)$ in the proposition. Note $0 < k^*(n,\beta) \leq 1$ for all $0 < \beta \leq 1$, and $n \geq 3$. Given concavity of the function, for $k \in [0,k^*(n,\beta))$, $(8 - 5\beta)(2 + k(n - 1)) - 4k^2n(2 - \beta) > 0$ and, hence, decentralization is preferred. On the other hand, for $k \in (k^*(n,\beta),1]$ centralization is preferred. ■

Proof of Proposition 4

(i) From (A19):

$$\frac{d\Pi_0^D(k,n,\beta)}{d\beta} = \frac{2(a - c_E - c_I)^2[4(1 - \beta)(2 + k(n - 1)) - k^2 n(4 - 3\beta)]}{[2 + k(n - 1)][2 + kn]^2[4 - \beta]^3}. \tag{A21}$$

Setting the first-order condition in (A21) equal to 0, and solving for β yields $\beta^*(k,n)$.

(ii) We first determine the firm's optimal transfer-pricing policy under decentralization, one where the firm commits upfront to t. Given t and p, the production decisions are as in (A3) and (A4). Next, given t, the supplier picks p that solves Equation (5). The first-order condition of Equation (5) yields $p^D(t)$ as follows:

$$p^D(t) = \frac{(a + c_E - t)(2 - k) + kn(2c_E + c_I - t)}{2[2 + k(n - 1)]}. \tag{A22}$$

Using (A3), (A4), and (A22), the firm picks t to solve:

$$\underset{t}{Max}\; q_0^D(p^D(t),t)[a - q_0^D(p^D(t),t) - nkq_i^D(p^D(t),t)] - q_0^D(p^D(t),t)[p^D(t) + c_I]. \tag{A23}$$

The first-order condition of (A23) yields the following t-value:

$$t^*(k,n) = c_I + \frac{(a - c_E - c_I)[2 - k][2 - k + kn(1 - k)]}{12 + 12k(n - 1) - 2k^3n(n - 1) + k^2(3 - 10n + 3n^2)}. \tag{A24}$$

From (A18), and the $\beta^*(k,n)$ expression in the proposition, it is easy to check that the t in (A24) is equal to $t^D(k,n,\beta^*(k,n))$. This completes the proof of Proposition 4. ■

Proof of Lemma 3

Suppose firm 0 is centralized. Due to price competition in the upstream market, the input is supplied to firm 0 by the least-cost supplier at a price no more than $c_E + \Delta$. Thus, in equilibrium, the low-cost supplier charges $p^C(k,n)$ if $p^C(k,n) < c_E + \Delta$, and $c_E + \Delta$, otherwise. That is, $p^C(\Delta,k,n) = Min\{p^C(k,n), c_E + \Delta\}$. Comparing the two terms in the minimization expression implies that $p^C(\Delta,k,n) = p^C(k,n)$ if $\Delta > \Delta_H$. In this case, from Lemma 1, $\Pi_0^C(\Delta,k,n) = \Pi_0^C(k,n)$. On the other hand, if $\Delta \leq \Delta_H$, $p^C(\Delta,k,n) = c_E + \Delta$. In this case, using (A1) and (A2), the Cournot quantities are $q_0^C(c_E + \Delta)$ and $q_i^C(c_E + \Delta)$. Substituting these quantities, and $p = c_E + \Delta$, in Equation (1), yields:

$$\Pi_0^C(\Delta,k,n) = \Pi_0^C(k,n)\left[2 - \frac{\Delta}{\Delta_H}\right]^2.$$

Suppose firm 0 is decentralized. In this case, using the same logic as before, $p^D(\Delta,k,n) = Min\{p^D(k,n), c_E + \Delta\}$. Comparing the two terms in the minimization expression implies that $p^D(\Delta,k,n) = p^D(k,n)$ if $\Delta > \Delta_L$. In this case, from Lemma 2, $\Pi_0^D(\Delta,k,n) = \Pi_0^D(k,n)$. On the other hand, if $\Delta \leq \Delta_L$, then $p^D(\Delta,k,n) = c_E + \Delta$. In this case, using (A3) and (A4), the Cournot quantities are $q_0^D(c_E + \Delta,t)$ and $q_i^D(c_E + \Delta,t)$. Using $q_0^D(c_E + \Delta,t)$ for $q_0^D(p,t)$ in Equation (6), and solving the first-order condition, yields the transfer price charged by the upstream division:

$$t^D(\Delta,k,n) = c_I + \frac{(a - c_E - c_I)(2 - k)}{2[2 + k(n - 1)]} - \frac{\Delta}{2}.$$

Finally, substituting $q_0^D(c_E + \Delta,t^D(\Delta,k,n))$ and $q_i^D(c_E + \Delta,t^D(\Delta,k,n))$ in Equation (1), yields:

$$\Pi_0^D(\Delta,k,n) = \Pi_0^D(k,n)\left[\frac{3}{2} - \frac{\Delta}{2\Delta_L}\right]^2.$$ ■

Proof of Corollary 3

Using the profit expressions from Lemma 3:

For $\Delta \leq \Delta_L$:

$$\begin{aligned}
&\frac{d[\Pi_0^D(\Delta,k,n) - \Pi_0^C(\Delta,k,n)]}{d\Delta} \\
&= \frac{[(a - c_E - c_I)(2 - k) - \Delta(2 + k(n - 1))][2 + k(n - 1) + k^2n]}{2(2 - k)^2(2 + kn)^2} \\
&\geq \frac{[(a - c_E - c_I)(2 - k) - \Delta_L(2 + k(n - 1))][2 + k(n - 1) + k^2n]}{2(2 - k)^2(2 + kn)^2} \\
&= \frac{(a - c_E - c_I)[2 + k(n - 1) + k^2n]}{3(2 - k)(2 + kn)^2} > 0.
\end{aligned}$$

For $\Delta_L < \Delta < \Delta_H$:

$$\begin{aligned}&\frac{d[\Pi_0^D(\Delta,k,n) - \Pi_0^C(\Delta,k,n)]}{d\Delta}\\ &= \frac{2[(a - c_E - c_I)(2 - k) - \Delta(2 + k(n - 1))][2 + k(n - 1)]}{(2 - k)^2(2 + kn)^2}\\ &\geq \frac{2[(a - c_E - c_I)(2 - k) - \Delta_H(2 + k(n - 1))][2 + k(n - 1)]}{(2 - k)^2(2 + kn)^2}\\ &= \frac{(a - c_E - c_I)[2 + k(n - 1)]}{(2 - k)(2 + kn)^2} > 0.\end{aligned}$$

For $\Delta \geq \Delta_H$:

$$\frac{d[\Pi_0^D(\Delta,k,n) - \Pi_0^C(\Delta,k,n)]}{d\Delta} = 0. \qquad \blacksquare$$

Proof of Proposition 5

We consider three cases.

Case 1

Suppose $\Delta_L < \Delta < \Delta_H$.

From Lemma 3, decentralization is preferred if and only if:

$$f(\Delta,k,n) = \Pi_0^D(k,n) - \Pi_0^C(k,n)\left[2 - \frac{\Delta}{\Delta_H}\right]^2 > 0. \tag{A25}$$

The function f is concave in Δ, $\frac{d^2f(\Delta,k,n)}{d\Delta^2} < 0$, and is increasing in Δ in this interval, $\left.\frac{df(\Delta,k,n)}{d\Delta}\right|_{\Delta=\Delta_H} > 0$. Moreover, $f(\Delta_L,k,n) < 0$. Hence, a necessary condition for (A25) to hold is that $f(\Delta_H,k,n) > 0$. From the continuity of the profit function, the condition $f(\Delta_H,k,n) > 0$ is equivalent to the requirement that decentralization is preferred in Proposition 1. Hence, the conditions in Proposition 1 are necessary for (A25) to hold. Given $f(\Delta_L,k,n) < 0$, $f(\Delta_H,k,n) > 0$, and f is increasing it follows that there exists a $\Delta^*(k,n) \in (\Delta_L,\Delta_H)$ such that (A25) holds for all $\Delta > \Delta^*(k,n)$. Since f is concave (and increasing in the relevant region of Δ-values), $\Delta^*(k,n)$ is simply the smaller root of the quadratic equation $f(\Delta,k,n) = 0$. This is the $\Delta^*(k,n)$ value presented in the proposition. Thus, the conditions in Proposition 1 and $\Delta > \Delta^*(k,n)$ are necessary and sufficient for (A25) to hold.

Case 2

Suppose $\Delta \leq \Delta_L$.

For these Δ-values, the conditions in the proposition imply decentralization is not optimal. This follows from the fact that $\Delta_L < \Delta^*(k,n)$. This is the correct conclusion since for $\Delta \leq \Delta_L$, from Lemma 3, firm 0's profits are higher under centralization:

$$\Pi_0^C(k,n)\left[2-\frac{\Delta}{\Delta_H}\right]^2-\Pi_0^D(k,n)\left[\frac{3}{2}-\frac{\Delta}{2\Delta_L}\right]^2$$
$$=\frac{[(a-c_E-c_I)(2-k)-\Delta(2+k(n-1))]^2[2+k(n-1)+k^2n]}{4(2-k)^2(2+kn)^2[2+k(n-1)]}>0.$$

Case 3

Suppose $\Delta \geq \Delta_H$.

For these Δ-values, from Lemma 3, firm 0's profit equals $\Pi_0^C(k,n)$ under centralization and $\Pi_0^D(k,n)$ under decentralization. Hence, Proposition 1 presents the necessary and sufficient condition for decentralization to be preferred. The conditions in Proposition 5 reduce to precisely those in Proposition 1. In particular, for $n \leq 2$, $\Delta > \Delta^*(k,n)$ since $\Delta_H > \Delta^*(k,n)$. Also, for $n > 2$ and $k < k^*(n)$, $\Delta > \Delta^*(k,n)$ since $k < k^*(n)$ ensures $\Delta_H > \Delta^*(k,n)$. ■

Proof of Proposition 6

Using Table 1, if firm 1 opts for centralization, firm 0 is better off under decentralization as shown in (A26):

$$\frac{(a-c_E-c_I)^2(6-k^2)}{18(2+k)^2}-\frac{(a-c_E-c_I)^2}{4(2+k)^2}=\frac{(a-c_E-c_I)^2(3-2k^2)}{36(2+k)^2}>0. \tag{A26}$$

Also, if firm 1 opts for decentralization, firm 0 is again better off under decentralization as shown in (A27):

$$\frac{(a-c_E-c_I)^2(12-2k^2)}{(2+k)^2(6-k)^2}-\frac{(a-c_E-c_I)^2(6+k)^2}{144(2+k)^2}$$
$$=\frac{(a-c_E-c_I)^2(432-216k^2-k^4)}{144(2+k)^2(6-k)^2}>0. \tag{A27}$$

From (A26) and (A27), decentralization is a dominant choice for firm 0. By symmetry, the same is true for firm 1. ■

Proof of Proposition 7

Suppose r firms out of the $n + 1$ (symmetric) firms are decentralized. In this case, let $p_i^C(k,n,r)$ denote the price the supplier charges firm i if the firm is centralized; the price is $p_i^D(k,n,r)$ if firm i is decentralized. Similarly, the centralized firm's quantity and profits are denoted by $q_i^C(k,n,r)$ and $\Pi_i^C(k,n,r)$, respectively; for the decentralized firm, the corresponding notation is $q_i^D(k,n,r)$ and $\Pi_i^D(k,n,r)$. Finally, the transfer price charged by the upstream division of a decentralized firm is denoted by $t_i^D(k,n,r)$.

Some tedious algebra can be used to confirm that the solution presented below satisfies (i) the first-order condition of firm i with respect to q_i and (ii) the first-order conditions with respect to p_i for the supplier and with respect to t_i for the upstream divisions of decentralized firms:

$$p_i^C(k,n,r)=c_E+\frac{a-c_E-c_I}{2};$$

$$p_i^D(k,n,r)=c_E+\frac{(a-c_E-c_I)[4+k(3n-r-1)]}{12+k[6n-2r-4]};$$

$$
\begin{aligned}
t_i^D(k,n,r) &= c_I + \frac{(a - c_E - c_I)(2 - k)}{6 + k[3n - r - 2]};\\
q_i^C(k,n,r) &= \frac{(a - c_E - c_I)[6 + k(3n - 2)]}{2(2 + kn)[6 + k(3n - r - 2)]};\\
q_i^D(k,n,r) &= \frac{(a - c_E - c_I)[2 + k(n - 1)]}{(2 + kn)[6 + k(3n - r - 2)]};\\
\Pi_i^C(k,n,r) &= \frac{(a - c_E - c_I)^2[6 + k(3n - 2)]^2}{4(2 + kn)^2[6 + k(3n - r - 2)]^2}; \text{ and}\\
\Pi_i^D(k,n,r) &= \frac{(a - c_E - c_I)^2[12 + 12k(n - 1) + k^2(3n^2 - 8n + 3) - k^3n(n - 1)]}{(2 + kn)^2[6 + k(3n - r - 2)]^2}.
\end{aligned}
\tag{A28}
$$

Given (A28), we now consider when decentralization by the firms is an equilibrium. Given all other firms are decentralized, firm i's profit if it centralizes is $\Pi_i^C(k,n,n)$; if it decentralizes, its profit is $\Pi_i^D(k,n,n+1)$. Hence, firm i prefers to decentralize if and only if $\Pi_i^D(k,n,n+1) - \Pi_i^C(k,n,n) \geq 0$. Using (A28), this equilibrium condition can be written as $g(k,n) \geq 0$:

$$g(k,n) = h_0 + h_1k + h_2k^2 + h_3k^3 + h_4k^4 + h_5k^5, \text{ where}$$

$$
\begin{aligned}
h_0 &= 432;\\
h_1 &= 720(n - 1);\\
h_2 &= 12(37 - 92n + 37n^2);\\
h_3 &= 12(-10 + 53n - 53n^2 + 10n^3);\\
h_4 &= 12 - 164n + 303n^2 - 164n^3 + 12n^4; \text{ and}\\
h_5 &= -16(n - 1)^3n.
\end{aligned}
\tag{A29}
$$

From (A29), $g(k,0) = 12(2 - k)^2(3 - k)^2 > 0$, so in the monopoly setting ($n = 0$), as expected, the firm opts for decentralization. For $n \geq 1$, the h-coefficients of the 5th degree polynomial in k, $g(k,n)$, change sign only once and, hence, from Descartes' rule of signs, there is at most one positive real root. Also, $g(0,n) = 432 > 0$. Hence, for any $n \geq 1$, if $g(1,n) \geq 0$, then $g(k,n) \geq 0$, and decentralization by the firms is an equilibrium for all k. On the other hand, if $g(1,n) < 0$, then there exists a unique $\bar{k}(n)$ such that decentralization by all firms is an equilibrium if and only if $k < \bar{k}(n)$.

Using (A29), $g(1,n) = 48 + 104n + 63n^2 + 4n^3 - 4n^4$. This function is positive for $n \leq 5$, and negative for $n > 5$, proving part (i) and the cutoff property in part (ii) of the proposition. Finally, it is easy to check that $g(k^*(n),n) > 0$ for $n > 5$ and, thus, $\bar{k}(n) > k^*(n)$ for such n-values. ■

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THE ACCOUNTING REVIEW
Vol. 82, No. 3
2007
pp. 581–619

Who Trades on Pro Forma Earnings Information?

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ABSTRACT: In recent years, many companies have emphasized adjusted-GAAP earnings numbers in their quarterly press releases. While managers use different names to describe these nonstandard earnings metrics, the financial press frequently refers to them as "pro forma" earnings. Managers and other advocates of pro forma reporting argue that these disclosures provide a clearer picture of companies' core earnings. On the other hand, regulators, policymakers, and the financial press often allege that managers' pro forma earnings disclosures are opportunistic attempts to mislead investors. Recent evidence suggests that while many pro forma earnings disclosures are altruistically motivated, some may represent managers' attempts to portray overly optimistic financial performance. If this is the case, then less wealthy, less sophisticated, individual investors are arguably the most at risk of being misled. Consequently, this study investigates *who* trades on pro forma earnings information. Our intraday investigation

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We thank Steve Baginski, Linda Bamber, Hank Bessembinder, Cal Boardman, Christine Botosan, David Hirshleifer, Karl Lins, Mike Lemmon, Srinivasan Sankaraguruswamy, Kay Stice, Beverly Walther, workshop participants at Brigham Young University, Duke University, Emory University, Mellon Capital Management, University of Notre Dame, Southern Methodist University, the Batten Forum at the College of William & Mary, and participants at the 2005 FARS Midyear Meeting for helpful suggestions. We are grateful to two anonymous reviewers and Patricia Dechow (editor) for their many constructive suggestions. We appreciate the valuable research assistance of Chad Larson, Kris Allee, Joe Bartlett, Dirk Black, Bryant Blanchard, Brandon Buhler, Leo Ebbert, Jonathan Fife, Josh Gagon, Kirk Gibb, Bryan Graden, Alicia Ingalls, Robert Judd, Jon Liljegren, Sam Mautz, Greg Packer, Heidi Prescott, John Prete, Willis Pueblo, Rob Shaw, Jacob Smith, Scott Tandberg, Patrick Walsh, Adam Ward, Chris Williams, and Devin Williams. We are also grateful to Thomson Financial for providing earnings forecast data, available through the Institutional Brokers Estimate System. These data have been provided as a part of their broad academic program to encourage earnings expectation research. Professors Christensen and Black express appreciation to Brigham Young University for funding that made this research possible. Professor Bhattacharya acknowledges financial support provided by Cox School of Business at the Southern Methodist University.

Editor's note: This paper was accepted by Patricia Dechow.

Submitted July 2004
Accepted September 2006

of transactions around earnings announcements containing pro forma earnings information reveals that less sophisticated investors' announcement-period abnormal trading is significantly positively associated with the magnitude and direction of the earnings surprise based on pro forma earnings. In contrast, we find no association between sophisticated investors' trading and manager-reported pro forma information. Overall, our analyses and numerous robustness tests suggest that the segment of the market that relies on pro forma earnings information is populated predominantly by less sophisticated individual investors. This evidence is particularly relevant to standard-setters and regulators given that Section 401(b) of the Sarbanes-Oxley Act of 2002 and subsequent SEC regulations are specifically designed to protect ordinary investors from misleading pro forma information.

Keywords: *pro forma earnings; corporate disclosure; The Sarbanes-Oxley Act of 2002; SEC regulations.*

Data Availability: *The data are available from the public sources identified in the text.*

I. INTRODUCTION

The managerial practice in recent years of reporting a nonstandard, alternative profitability measure in the same press release with the audited earnings number has generated substantial controversy and debate. These alternative profitability metrics (popularly known as "pro forma" earnings) are GAAP earnings adjusted for items that managers deem to be transitory or non-representative of future earnings (Weil 2001).[1] Managers defend this practice by asserting that pro forma earnings figures provide stakeholders a more accurate assessment of sustainable operating performance (i.e., a better measure of core earnings) than do standard GAAP earnings figures (Bray 2001). On the other hand, policymakers, regulators, and the financial press often allege that pro forma earnings are incomplete, inaccurate, and misleading to investors (Derby 2001; Dreman 2001; Elstein 2001). This debate highlights the possibility that, while some pro forma reports are altruistically motivated, others may represent managers' attempts to divert stakeholders' attention from poor operating performance by excluding bad news from GAAP earnings.[2]

Recent research examining market reactions to earnings announcements containing pro forma information provides evidence that investors often pay more attention to management-adjusted pro forma earnings numbers than to audited GAAP earnings figures (Bhattacharya et al. 2003 [hereafter BBCL]; Lougee and Marquardt 2004). Further, two recent experimental studies (Frederickson and Miller 2004; Elliott 2006) provide evidence that less sophisticated investors are more likely to rely on pro forma information than more sophisticated investors. Finally, a small but growing stream of research provides evidence that pro forma earnings information may be misleading to investors. For example, Bowen et al. (2005) provide evidence that managers strategically emphasize pro forma earnings metrics in earnings press releases, while Doyle et al. (2003) argue that investors are misled because they fail to understand that certain expenses excluded from analysts' "street earnings" are actually associated with lower levels of future firm performance.[3] Collectively, this body of evidence suggests that (1) investors pay attention to pro forma earnings information, (2) more and less sophisticated investors likely process this information differently, and (3) some adjusted-GAAP disclosures may be misleading to investors. Consequently, this study investigates *who* trades on pro forma earnings information—sophisticated investors (e.g., institutions), less sophisticated individual investors, or both. Since individual investors with a limited ability to access and process information are most at risk of being misled, our investigation is timely and relevant given the recent concern of legislators and regulators that pro forma information may mislead investors.

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[1] Managers do not always label their adjusted-GAAP earnings numbers as "pro forma" income. Press releases often describe adjusted-GAAP earnings figures using terms such as cash earnings, core earnings, adjusted earnings, earnings excluding certain items, or earnings before certain items. Wallace (2002) provides an exhaustive list of common nomenclatures managers use to label their adjusted-GAAP earnings metrics. While our sample includes all of the various adjusted-GAAP earnings labels described by Wallace (2002), we refer to them throughout the paper using the terms "pro forma" or "adjusted-GAAP" earnings for ease of exposition.

[2] Prior research provides a more detailed discussion of this debate (e.g., Bhattacharya et al. 2003; Lougee and Marquardt 2004; Elliott 2006; among others). While we only discuss this issue briefly, we emphasize that there has been considerable deliberation on both sides of the matter, and we believe both positions have merit. It is likely that managers often present pro forma earnings with a legitimate incentive to provide a clearer picture of recurring earnings. For example, Bhattacharya et al. (2003) find evidence that some of their pro forma disclosures contain adjustments where managers voluntarily exclude one-time gains. On the other hand, Bowen et al.'s (2005) evidence suggests that managers strategically emphasize the earnings metric that "spins" the more favorable story.

We use detailed transactions data from the Trades and Quotes (TAQ) database to examine investor trading responses around 5,736 quarterly earnings press releases containing voluntarily disclosed pro forma earnings numbers issued between January 1998 and December 2003. Following prior research (e.g., Cready 1988; Lee 1992; Lee and Radhakrishna 2000; Bhattacharya 2001; Bushee et al. 2003), we use trade size to distinguish larger, sophisticated, and well-informed investors from smaller, less sophisticated, and less informed investors. We rely on the Lee-Ready algorithm to infer trade direction (buy versus sell) from intraday transactions data to allow us to examine net buying (i.e., buy minus sell) activities of more versus less sophisticated investors around earnings press releases containing pro forma earnings information (e.g., Lee and Ready 1991).

The results suggest that the market reaction to pro forma earnings information is almost exclusively attributable to the activities of less sophisticated investors. The results indicate that less sophisticated investors' abnormal net-buying activities are significantly positively associated with the magnitude and direction of the forecast error based on pro forma earnings (i.e., managers' pro forma income number minus the market's earnings expectation). In other words, we find that the higher (lower) the pro forma forecast error, the higher (lower) the less sophisticated investors' abnormal net-buying activities, suggesting that these investors buy and sell shares based on information in pro forma earnings. In addition, less sophisticated investors' abnormal net-buying activities are more highly associated with the pro forma forecast error than with the GAAP operating earnings forecast error (i.e., the GAAP operating EPS figure minus the market's earnings expectation). We also find that less sophisticated investors' trading activities are significantly positively associated with the I/B/E/S forecast error (i.e., the actual EPS "street earnings" figure published by I/B/E/S minus the market's earnings expectation). However, the pro forma forecast error appears to be incrementally informative (after controlling for both GAAP operating earnings and I/B/E/S actual earnings forecast errors) in explaining the trading activities of less sophisticated investors.[4] In sharp contrast, more sophisticated investors' abnormal net-buying activities are not associated with forecast errors based on pro forma or GAAP

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[3] While this study focuses on manager-adjusted earnings numbers, prior research investigates adjusted-GAAP earnings numbers disclosed by managers as well as analysts. Gu and Chen (2004) introduce a useful convention by labeling manager-adjusted earnings figures voluntarily reported in earnings press releases as "pro forma" earnings, and analyst-adjusted earnings numbers reported by forecast tracking services as "street" earnings. Following Gu and Chen (2004), we report management-reported numbers as "pro forma," or "adjusted-GAAP" earnings and the numbers published by forecast data providers as "street" earnings.

[4] Likewise, the I/B/E/S forecast error is significantly incrementally informative (after controlling for both the GAAP operating earnings and pro forma forecast errors) in explaining less sophisticated investors' trading activities.

operating earnings. We, however, find some evidence that more sophisticated investors' abnormal trading activities are significantly positively associated with the I/B/E/S forecast error. Taken together, these results suggest that the segment of the market that relies on pro forma earnings information is likely to be disproportionately populated by less sophisticated investors.

A potential caveat in interpreting results reported in this literature arises from the classic errors-in-variables problem (e.g., Bradshaw 2003; BBCL; Berger 2005). This problem arises because forecast tracking services, such as I/B/E/S, attempt to exclude the same items from their "street" earnings number that analysts exclude from their forecasts. Thus, the "street" forecast error generally has less measurement error than forecast errors computed by subtracting the mean analysts' forecast from GAAP operating earnings or manager-adjusted pro forma earnings. In order to ensure that our results are not attributable to measurement error, we follow Gu and Chen (2004) in disaggregating the earnings number into components and investigating managers' incremental adjustments beyond those made by analysts. This disaggregation allows us to operationalize an alternative specification that circumvents the errors-in-variables problem.[5] Consistent with our main results, we find that after controlling for the I/B/E/S forecast error (the most accurate proxy for the earnings surprise), less sophisticated investors' (but not more sophisticated investors') abnormal net-buying activities are positively associated with managers' *incremental* earnings adjustments.

Our evidence that less informed, less sophisticated investors trade on pro forma earnings information while more sophisticated investors do not has important implications for regulators—whose objectives include the protection of ordinary investors from potentially inaccurate and misleading information and the leveling of the informational playing field.[6] Specifically, Section 401(b) of the Sarbanes-Oxley Act of 2002 (SOX) directs the SEC to issue regulations to ensure that pro forma financials are not misleading to ordinary investors. The SEC subsequently issued Regulation G in January 2003 to implement the provisions of SOX. Our evidence highlights the importance of continued monitoring of the effectiveness of Regulation G and assessing the need for additional or alternative regulatory actions.

The rest of the paper is organized as follows. Section II discusses the background and the research questions. Section III describes the data and the sample selection criteria. Section IV explains the research design and the empirical proxies. Section V discusses the results, and finally Section VI provides concluding remarks.

II. BACKGROUND AND RESEARCH QUESTIONS

The rapid proliferation of pro forma financial disclosures in recent years has fueled an intense debate among supporters and critics of pro forma reporting. On the one hand, managers who report pro forma earnings numbers claim that pro forma earnings represent an improved metric for assessing future cash flows and firm value since pro forma earnings numbers exclude transitory and noncash items from GAAP earnings (e.g., Bray 2001; Weil 2001). Some practitioners concur with managers' claims that removing noncash and nonrecurring items enhances comparability in time-series measures because the GAAP measure includes items such as restructuring charges and gains and losses on the sale of assets, which have little implication for future earnings (Halsey and Soybel 2002). Some equity analysts also echo this sentiment as one analyst comments that the reported GAAP figure is usually an "accounting fiction" because it frequently includes nonrecurring items and other accrual accounting distortions (MacDonald 1999). BBCL document instances where managers exclude one-time gains that results in a pro forma number that is lower than the GAAP number. These scenarios represent a conservative estimate of future profitability. Such examples suggest that some pro forma reports are altruistically motivated to disclose a clearer picture of core earnings. Richard Bernstein, chief U.S. strategist for Merrill Lynch & Co., summarizes the popularity of pro forma reporting as follows, "But for all the now-obvious shortcomings, pro forma reporting remains big in the tech world" (Sender 2002).

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[5] Marques (2006) uses a similar decomposition of earnings components. We discuss the Gu and Chen (2004) framework and this alternative specification in our sensitivity analyses.

[6] One of the SEC's primary missions is to mandate disclosures that reduce the extent to which some investors are at an informational disadvantage relative to others (Foster 1986, 40; Hand and Beatty 1992). The AICPA study group on the *Objective of Financial Statements* notes, "An objective of financial statements is to serve primarily those users who have limited authority, ability or resources to obtain information" (AICPA 1973, 17).

Legislators, regulators, and the financial press, on the other hand, have alleged that managers opportunistically and selectively exclude income statement items from audited GAAP earnings in order to portray the company in the most favorable light possible (Derby 2001; Dreman 2001; Elstein 2001; Liesman and Weil 2001a, 2001b). Critics of pro forma reporting are skeptical of managers' claims that their adjusted earnings metrics provide a clearer picture of sustainable "core earnings." Former SEC Chairman Harvey Pitt commented, "Without appropriate disclosure, no investor—certainly not any ordinary investor—can read these (pro forma financials) in a way that's useful. An investor can't know what's been left out, why it's left out, or how it compares with other companies' earnings" (Levinsohn 2002). The Financial Accounting Standards Board (FASB) has also expressed concern that the proliferation of pro forma earnings is undermining the quality of financial reporting (FASB 2002). In his 2002 letter to Berkshire Hathaway shareholders, Warren Buffett warned, "[C]ompanies issuing such [pro forma] numbers want you to unthinkingly accept concepts that are dangerously flawed" (Byrnes and Derhovanesian 2002). Lawrence Summers of the U.S. Treasury advises investors not to pay attention to pro forma figures but to rely on audited GAAP earnings instead (Wessel 2002). Some academics also share this concern. D'Avolio et al. (2002) argue that even though GAAP numbers may not always provide economically superior information, failure to follow these standards is likely to lead to inefficient overall outcomes as the ability of regulators to enforce disclosure standards deteriorates.

Despite the skepticism expressed by standard-setters, regulators, and legislators, recent research finds evidence that investors pay attention to various adjusted-GAAP earnings figures. Bradshaw and Sloan (2002) and Brown and Sivakumar (2003) report that investors attach more weight to street earnings figures published by a major analyst forecast tracking service (I/B/E/S) than to GAAP earnings numbers. Lougee and Marquardt (2004) provide evidence that manager-reported pro forma income has incremental information content over GAAP earnings. BBCL report that investors not only pay attention to manager-adjusted pro forma earnings, but they focus on pro forma earnings significantly more than GAAP operating earnings (before special items). These results suggest that at least some investors rely on adjusted-GAAP earnings numbers published by managers.

Two recent experimental studies find evidence that less sophisticated investors may interpret pro forma earnings information differently from sophisticated, professional investors. Frederickson and Miller (2004) find that less sophisticated investors (M.B.A. students) predict higher future stock prices than more sophisticated investors (security analysts) when they see a press release containing a pro forma earnings number that exceeds the GAAP earnings figure. Elliott (2006) reports that when the pro forma earnings number is emphasized in the press release relative to the GAAP earnings number, less-sophisticated investors (M.B.A. students) increase their expectations about future earnings, while more sophisticated investors' (analysts) judgments are unaffected by this manipulation. Collectively, these results suggest that (1) investors pay attention to pro forma numbers, and (2) less sophisticated, less informed individual investors are likely to process pro forma earnings information differently from better informed professional investors.

Several recent studies suggest that pro forma earnings information may be misleading to investors. Doyle et al. (2003) find that expenses excluded from analysts' "street earnings" have implications for future cash flows. They also find that investors fail to fully understand the implications of these exclusions for future firm performance, and a trading strategy based on the excluded expenses generates significant abnormal returns in the future periods even after controlling for known risk proxies. Lougee and Marquardt (2004) also provide preliminary evidence that investors misprice management-issued pro forma numbers. While Johnson and Swartz (2005) find some evidence that "pro forma" firms are systematically priced higher than "non-pro-forma" firms, they fail to find consistent evidence indicating that investors are misled by pro forma earnings information. Finally, Bowen et al.'s (2005) results suggest that managers' placement of pro forma versus GAAP earnings metrics in earnings press releases is opportunistically motivated and focuses on the metric that "spins" the more favorable story.

Given preliminary evidence that pro forma earnings may be misleading, the investors who are clearly most at risk of being misled are the less wealthy, individual class of investors because extant research indicates that these investors lack the necessary sophistication and experience to fully understand the precision and reliability of their information set.[7] Regulators and legislators are particularly concerned that pro forma disclosures may be misleading to ordinary investors. The U.S. Congress has expressed serious concern that inaccurate or misleading corporate disclosures may hurt the less sophisticated investors (Burns 2001). Therefore, a thorough investigation of *who* trades on pro forma earnings information is particularly timely and relevant. Consequently, this study examines investor trading responses around pro forma earnings announcement dates. Specifically, the study investigates the following research questions:

RQ1: Who trades on pro forma earnings information: less sophisticated investors, more sophisticated investors, or both?

RQ2: To what extent do more and less sophisticated investors trade incrementally on the earnings surprise based on pro forma earnings vis-à-vis the surprise based on GAAP operating earnings or the earnings figure published by I/B/E/S?

III. SAMPLE SELECTION AND DATA

We searched the PR Newswire and Business Wire on LexisNexis for the years 1998–2003 to collect a comprehensive sample of pro forma press releases. A typical pro forma press release contains the GAAP earnings per share (EPS) figure, a pro forma earnings number (an adjusted-GAAP earnings measure voluntarily disclosed by managers) for the current quarter, and various other details management deems to be relevant. We include earnings announcements in which the company discloses a pro forma number that differs from the "bottom line" GAAP diluted EPS number disclosed in the same press release. Our original search uses the keywords "pro forma," "pro-forma," and "proforma" and retrieves 50,011 press releases. However, companies often use other nomenclatures to describe their adjusted-GAAP earnings figures. Wallace (2002) performs a detailed categorization of adjusted-GAAP earnings nomenclatures used by companies. Based on Wallace's (2002) list of adjusted-GAAP earnings nomenclatures, we further search LexisNexis using the following expanded search string: "earnings excluding," "net income excluding," "adjusted net income," "adjusted loss," "cash earnings," "earnings before," "free cash flow," "normalized EPS," "normalized earnings," "recurring earnings," "distributable cash flow," "GAAP one-time adjusted," "GAAP adjusted," "cash loss," AND NOT "pro forma," "pro-forma," or "proforma."[8] This expanded search yields an additional 33,373 hits bringing the grand total to 83,384 potential press releases. After carefully reading each press release, we find that 17,511 announcements contain actual quarterly pro forma earnings announcements. The other 65,873 press releases from the initial searches refer to such things as current period pro forma revenues, forward-looking pro forma forecasts, earnings after adding in results from firms acquired or merged in the current period, or statements referring to prior period pro forma earnings.

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[7] In an experimental setting, Bloomfield et al. (1999) find that less sophisticated investors do not fully understand the limitations of their information set, and trade aggressively to systematically transfer wealth to sophisticated investors. Barber and Odean (1999, 2000) conclude after analyzing proprietary brokerage data that individual investors make suboptimal trading decisions and earn returns far below the market average due to their inability to assess the limitations of their information sets. Hirshleifer and Hong (2003) develop a stylized analytical framework to model sophisticated and naïve investors' interactions in the context of pro forma disclosure and show that naïve investors do not appropriately discount nonstandard pro forma information and consequently overvalue the firm, while sophisticated investors do not. Stock price, as a weighted average of beliefs, appears high to sophisticated investors prompting them to sell and appears low to naïve investors prompting them to buy. Consequently, a wealth transfer takes place between the two groups.

We require firm-quarter observations to have data available in the Compustat, CRSP, I/B/E/S, and TAQ databases in order to perform our empirical analyses. These requirements result in a final sample of 5,736 announcements of 2,209 unique firms from January 1998 to December 2003. We collect actual pro forma announcement time stamps from Bloomberg. If a company announces pro forma earnings during nontrading hours (i.e., between 4:30 p.m. and 9:30 a.m.), we set the time of the announcement to 9:30 a.m. the next trading day. We obtain detailed intraday transactions data from the TAQ database. TAQ reports all trades and quotes originating from the NYSE, AMEX, Nasdaq, or the regional exchanges. For each trade, TAQ provides the time of the transaction to the nearest second, price, volume, and a trade condition code. Except for the opening trade of each day, we include all trades with a condition code of "regular sale" occurring between 9:30 a.m. and 4:30 p.m. Eastern Standard Time (EST) in our tests.[9] We exclude the opening trade because it is often the sum of multiple orders and including it could add noise to our measures (Lee and Ready 1991; Lee 1992). However, our results are almost identical (not tabulated) when we include the opening trade in our analyses. We only include trades with a "regular sales" condition code because these trades result from continuous two-sided auctions involving market orders, limit orders, and buys and sells against the specialists' inventories. This is not the case when the condition code indicates something other than a "regular sale" (e.g., large block trades or stopped orders).

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[8] Note that we do not include EBIT or EBITDA since they are commonly reported as standard steps in the income statement. Moreover, these figures were often reported on a per share basis long before the pro forma reporting trend began in the late-1990s.

[9] Although NYSE and AMEX close at 4:00pm EST, we allow an extra 30 minutes to pick up trades originating on the regional exchanges or trades reported late (e.g., Lee and Ready 1991; Lee 1992; Bhattacharya 2001).

IV. RESEARCH DESIGN AND EMPIRICAL PROXIES

Observation Intervals

We examine three trading days surrounding the pro forma announcement date: day −1, day 0, and day +1.[10] In our analysis a "trading day" is comprised of seven consecutive trading hours (since a normal trading day from 9:30 a.m. to 4:30 p.m. is seven hours long). Day 0, the day of the earnings announcement containing a pro forma earnings figure, begins at the time of the press release and continues for the next seven hours during which equity markets are open. Since our trading days are defined with respect to the timing of the earnings press release, they generally do not correspond to a typical day of trade beginning at 9:30 a.m. and ending at 4:30 p.m. EST, but instead span seven hours of trading during two different calendar days.

Proxy for Investor Sophistication and Wealth

Prior research suggests that, on average, wealthier, more sophisticated, professional investors (like institutions) are likely to make larger trades, while less wealthy and less sophisticated investors (primarily individuals) are likely to make smaller trades (e.g., Easley and O'Hara 1987; Hasbrouck 1988, 1991; Chan and Lakonishok 1993; Lee and Radhakrishna 2000). Therefore, several prior studies (e.g., Cready 1988; Cready and Mynatt 1991; Lee 1992; Bhattacharya 2001), use trade size to differentiate wealthy, sophisticated investors from less wealthy, less sophisticated investors. Existing research also suggests that sophisticated, informed investors may not always submit large orders. Kyle's (1985) model shows that rational, informed investors often have an incentive to engage in medium-sized trades to disguise their private information. Empirical research also provides support for this conjecture (e.g., Cornell and Sirri 1992; Meulbroek 1992; Barclay and Warner 1993).

Sophisticated institutional investors, however, are unlikely to engage in very small trades as it may significantly reduce their trading profits for the following reasons. First, breaking a large order into numerous small orders significantly increases direct transaction costs. Second, a series of small orders from one account could prompt the specialist to increase the spread. Finally, breaking a large order into smaller parts would require more time to move all of the desired shares, thereby increasing the chance that other arbitrageurs would enter the market, further eroding trading profits. Consequently, small trades are likely to capture primarily individual trading activity, while medium and/or large trades are likely to capture primarily sophisticated institutional trading activity. Chakravarty's (2001) evidence from detailed audit trail data supports this conjecture. He finds that significant institutional trading activity takes place in medium-sized trades, while the vast majority of individual trading activity takes place in small trade sizes below 500 shares. Barclay et al. (2003) provide recent empirical evidence confirming Chakravarty's (2001) conclusions. Consequently, we investigate small trades to capture the activities of relatively less informed and less sophisticated individual investors and we examine medium and large trades to capture the activities of sophisticated, primarily institutional investors.

Our main analyses classify trades of $7,000 or less as small trades, trades between $7,000 and $50,000 as medium-sized trades, and trades over $50,000 as large trades. We also repeat all analyses (not tabulated) using several alternative cutoff schemes to ensure that our results are not sensitive to specific cutoff points. We use transactions of $5,000 or less, and less than $10,000 as alternative cutoff schemes for classifying small trades. We classify transactions between \$10,000 and \$50,000, and transactions between \$10,000 and \$100,000 as alternative cutoff points for medium trades. We use trades sizes of greater than or equal to \$40,000 and greater than or equal to \$100,000 to identify large trades. Finally, in order to examine extremely large orders that could only be submitted by institutions or extremely wealthy individuals, we examine a third alternative cutoff of \$250,000 or more for classifying large trades. All our results are qualitatively similar when we use these various alternative cutoff schemes.

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[10] We extend our examination up to five trading days before the earnings announcement date (i.e., one full week of trading prior to the earnings announcement). However, we detect no significant abnormal trading activities by more or less sophisticated investors before day −1.

Figure 1 provides the distributions of the average dollar value of shares traded[11] during each day in the announcement window. The purpose of this figure is to provide a snapshot of the activities of small, medium, and large investors during the announcement period. For example, we find that transactions of \$10,000 or lower (encompassing our various small-investor cutoffs) comprise approximately 29 percent of total dollar-volume on day 0, the earnings announcement day (second chart of Figure 1). Transactions between \$10,000 and \$50,000 (generally capturing medium-investor activities) also comprise approximately 29 percent of the total dollar-volume on the day of the announcement. Finally, transactions of \$50,000 or above (our large investor cutoff) comprise about 42 percent of raw dollar volume on the announcement day. We observe similar distributional characteristics on the day before (day -1) and the day after (day $+1$) the earnings announcement. Since our large-investor (medium-investor) cutoff captures approximately 46 percent (24 percent) of the daily announcement period *raw* dollar-volume during the three-day announcement window, we conclude that our sample does not consist primarily of smaller, thinly traded firms in whose stocks large investors seldom trade.

Method and Variable Definitions

As previously mentioned, we hand-collect adjusted-GAAP diluted earnings per share numbers disclosed by managers in their earnings press releases and label them as $EPS_{PROFORMA}$. We benchmark investor reactions to $EPS_{PROFORMA}$ with diluted earnings per share from operations ($EPS_{GAAP\text{-}OP}$) from Compustat.[12] This diluted operating EPS figure from Compustat excludes all "special items" and "below-the-line" items. Several recent studies document that investors pay significant attention to actual EPS figures provided by analyst forecast tracking services, such as I/B/E/S (e.g., Bradshaw and Sloan 2002; Brown and Sivakumar 2003; Doyle et al. 2003). Consequently, we examine a third earnings metric—the actual EPS figure from I/B/E/S ($EPS_{I/B/E/S}$).[13] Thus, our trading analysis complements the BBCL study that investigates price reactions to all three earnings metrics—pro forma EPS, GAAP operating EPS, and I/B/E/S actual EPS.

In order to ascertain the announcement-period investor reaction to new information contained in each of the three earnings metrics, we calculate the earnings surprise or forecast error for each metric using two different earnings expectations. The first expectation is the mean analysts' earnings forecast. This measure is timely (current) and comprehensive and has been used widely in empirical research as a proxy for the unobservable market

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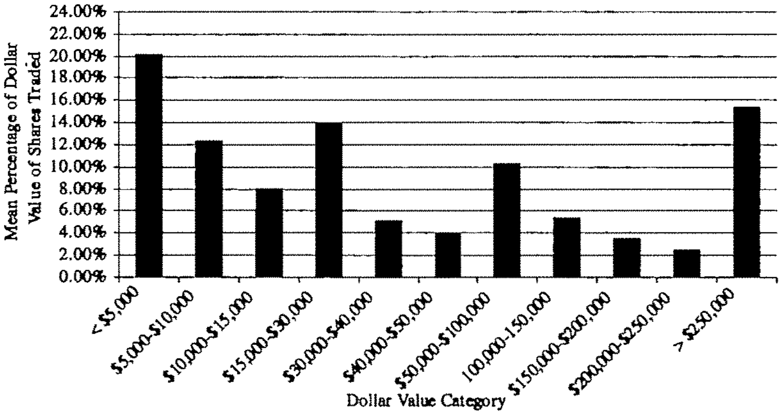
[11] Note that this figure illustrates *raw* dollar-volume, not *abnormal* dollar-volume.

[12] We begin with Compustat's basic earnings per share from operations (quarterly data item 177) and multiply this by the number of basic shares outstanding (Compustat quarterly data item 15) to get total operating earnings. We then divide operating earnings by the number of diluted shares outstanding (Compustat annual data item 171) to obtain quarterly diluted earnings per share from operations. Subsequent to our data collection for this project, we learned that Compustat's latest files contain a quarterly diluted operating EPS number (data item 181).

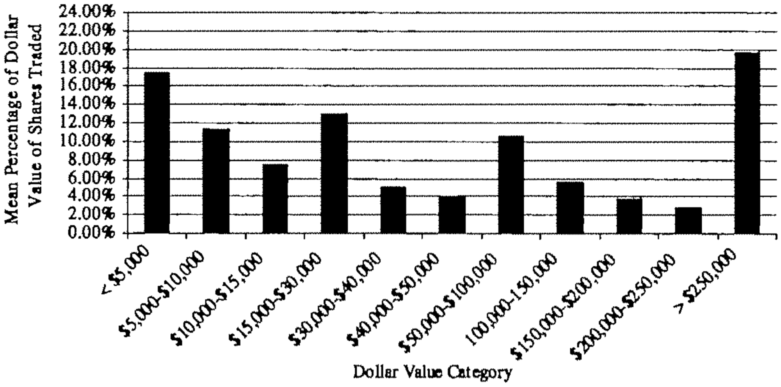
[13] We collect I/B/E/S actual EPS figures from the *unadjusted* I/B/E/S Actuals file. We use the unadjusted I/B/E/S Actual and Detail files to avoid biases arising from using *adjusted* I/B/E/S data (e.g., Payne and Thomas 2003).

FIGURE 1
Distribution of Average Dollar Value of Shares Traded during the Announcement Period

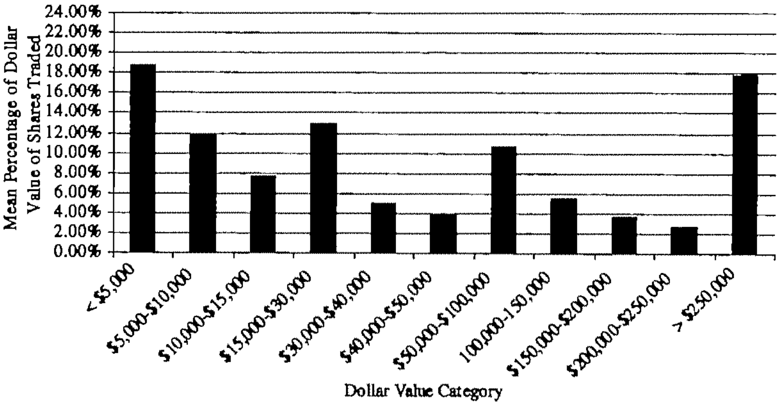
Distribution on Day - 1



Distribution on Day 0



Distribution on Day +1



earnings expectation. However, this measure may produce bias against the forecast error based on GAAP operating earnings when it is compared with forecast errors based on I/B/E/S and pro forma earnings figures. This bias stems from the fact that I/B/E/S generally excludes the same items from its actual EPS figures that the majority of analysts exclude from their forecasts. Further, there is overlap between exclusions made by managers and exclusions made by analysts (e.g., BBCL; Doyle et al. 2003). In order to address this concern, we employ a second earnings expectation—GAAP operating earnings from the same quarter of the previous year (i.e., the seasonal random-walk earnings expectation). Although the seasonal random-walk earnings expectation may alleviate the bias against the GAAP operating earnings forecast error, it is a much noisier expectation than the analysts' consensus forecast.[14] Thus, we compute three forecast errors ($FE_{PROFORMA}$, $FE_{GAAP\text{-}OP}$, and $FE_{I/B/E/S}$) by subtracting the earnings expectation (measured either by the analysts' mean forecast or by the seasonal random-walk forecast) from the three actual earnings metrics, and scaling this difference by the closing price five days before the earnings announcement date, day $t-5$ (e.g., Christie 1987).[15]

In order to examine the reaction of sophisticated versus less sophisticated investors to various earnings surprise metrics, we compute abnormal net order imbalance measures for the small-, medium-, and large-investor groups. We calculate small investors' abnormal net-buy volume (buy volume minus sell volume) for firm *i* on each day during the announcement period (day −1, day 0, or day +1) as the small investors' day *t* net-buy volume for firm *i minus* small investors' average daily non-announcement period net-buy volume for firm *i*, scaled by the average daily non-announcement period total trading volume for firm *i*. We call this measure as *SML_NETBUY*. Again, all trades of $7,000 or less are classified as small trades. The non-announcement period is a two-week period ending exactly one month before the earnings announcement date. Thus, *SML_NETBUY* is an abnormal measure of net-buying activity of small investors around earnings announcement dates. A positive value for this measure indicates above-normal buying activity by small investors during the event period. For example, when *SML_NETBUY* is regressed on $FE_{PROFORMA}$, a significantly positive coefficient on $FE_{PROFORMA}$ would indicate that when the pro forma forecast error is positive (i.e., good news based on pro forma earnings), small investors' net-buying activity increases, and when the pro forma forecast error is negative (i.e., bad news based on pro forma earnings), small investors' net-buying activity decreases. In other words, a positive and significant coefficient on $FE_{PROFORMA}$ would suggest that small investors trade in the direction of the pro forma earnings surprise. We compute *MED_NETBUY* exactly the same way but based on medium-sized trades between $7,000 and $50,000. Likewise, we compute *LRG_NETBUY* based on trades above $50,000. Our main analyses regress small, medium, and large abnormal net-buy volume measures separately on $FE_{GAAP\text{-}OP}$, $FE_{PROFORMA}$, and $FE_{I/B/E/S}$.[16]

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[14] Analysts' forecasts are more comprehensive and precise than random-walk forecasts because security analysts have incentives to quickly impound value-relevant predisclosure information in their earnings forecasts (e.g., Mikhail et al. 1999). Brown et al. (1987) argue that analysts forecasts are more accurate and less noisy than random-walk forecasts because analysts' forecasts have a contemporaneous advantage (i.e., incorporate a variety of other information than just the past earnings stream) as well as a timing advantage (i.e., they take into account more recent information).

[15] The mean forecast is calculated for each firm using all forecasts from the *unadjusted* I/B/E/S Detail file made within 90 days prior to the quarterly earnings announcement date. The 90-day restriction ensures that forecasts are current. We also repeat all analyses using the median forecast with no change in results. Further, we repeat our analyses using forecasts made within 45 or 60 days prior to the earnings announcement date. Shorter pre-announcement windows of 60 or 45 days ensure that forecasts are more current, but we lose observations as we reduce the length of the window (especially when we go to the 45-day window). The main tenor of the results, however, is unchanged when we use the shorter pre-announcement windows.

We rely on the algorithm developed by Lee and Ready (1991) to classify transactions as buys or sells. This algorithm uses a "tick" test to infer trade direction from intraday trades and quotes data obtained from the TAQ database and has been used widely in the empirical market microstructure literature.[17] A more recent paper by Ellis et al. (2000) suggests some modifications of the original Lee-Ready algorithm to improve its specification. We repeat our analyses (not tabulated) using the Ellis et al. (2000) procedure to classify trades as buys and sells, but find no qualitative difference in results.

Prior research suggests that small and large firms have different investor clienteles. Specifically, prior studies suggest that individual traders are likely to account for a greater proportion of the trading activity of smaller firms, while institutional traders are likely to account for a greater proportion of the trading activity of larger firms (e.g., Bhushan 1989; El-Gazzar 1998; Lee and Radhakrishna 2000). Consequently, when we regress small, medium, and large investors' abnormal net-buy volume measures on forecast errors based on pro forma earnings, GAAP operating earnings, or I/B/E/S actual earnings, we control for firm size in order to ensure that the results are not merely attributable to differences in firm size. We use the log of total assets (in millions) at the end of the previous quarter as our control for firm size (*SIZE*).[18] Finally, our abnormal net-buy measures control for small, medium, and large investors' firm-specific average level of liquidity trading, but they do not control for investors' trading responses associated with market-wide or macroeconomic factors. Therefore, we follow prior research (e.g., Bamber et al. 1997) by including a control for the influence of macroeconomic factors on investor net-buy responses in our regression models, the percentage of all NYSE/AMEX/Nasdaq firms' outstanding shares traded, *MKTVOL*, on each day in the announcement window.

V. RESULTS

Sample Characteristics

Evolution and Trends in Pro forma Reporting

Figure 2 illustrates trends in pro forma reporting practice during our sample period. The first chart indicates that pro forma reporting, in general, increased over time from 1998 until the middle of 2001 and has declined since that time. It is interesting to note that pro forma reporting peaked just prior to the major accounting scandals of 2001 and dropped dramatically in the third quarter of 2002—soon after the SOX, which requires explicit reconciliation between pro forma and GAAP earnings. The second chart presents trends for adjusted-GAAP earnings numbers for which managers use the "pro forma" label, while the third chart shows trends when managers use various other nomenclatures described in the Wallace (2002) monograph. For ease of exposition, we group these labels under the

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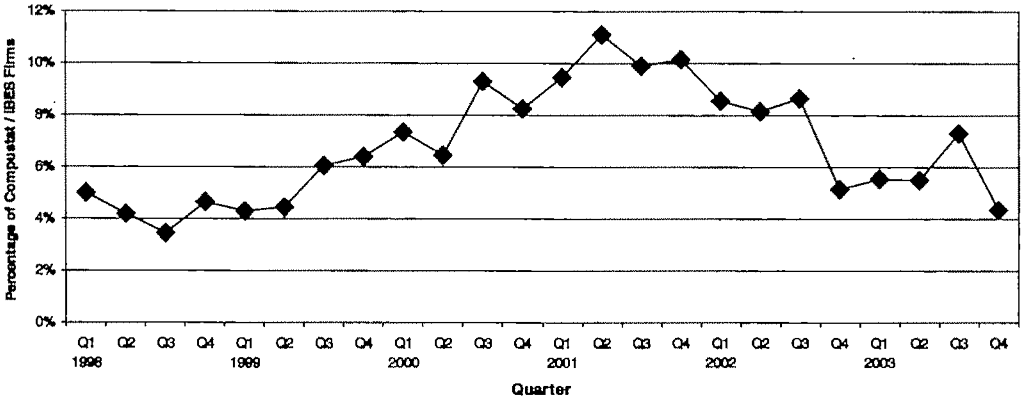
[16] We also repeat our analyses using *unsigned* abnormal trading volume instead of *signed* net-buy volume. These analyses are less reliable because the use of *unsigned* trading volume metrics requires that we take the absolute value of the forecast error variables. Thus, trading volume analysis ignores valuable information (for example, such analysis does not make any qualitative distinction between "good news" or positive forecast error and "bad news" or negative forecast error) and introduces noise in the results. Nevertheless, while the *unsigned* trading volume results are weaker, the main tenor of the results remains unchanged.

[17] The Lee-Ready algorithm ignores the current quote if it is less than 5 seconds old and compares the current trade price with the bid and ask of the previous quote (which is then assumed to be the current quote) to infer trade direction. Studies since then have used a 5-second lag, a 2-second lag, or no lag to define the current quote. Our main analyses employ the 5-second lag to define the current quote as in Lee and Ready (1991), but we repeated all our analyses using the 2-second lag, or no lag with no qualitative change in the results.

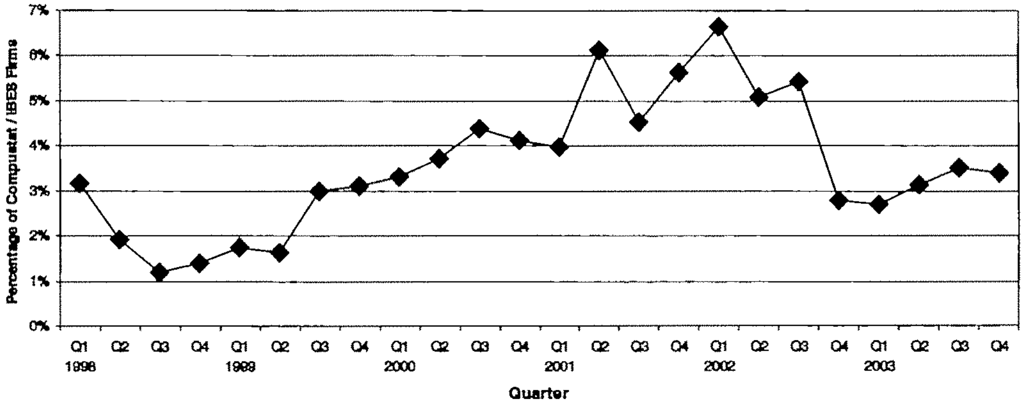
[18] We repeat our analyses using market value of common equity five days prior to the earnings announcement date and net sales from the previous quarter as alternative measures of firm size. The results are qualitatively similar.

FIGURE 2
The Percentage of Firms on both Compustat and I/B/E/S Reporting Adjusted-GAAP Earnings

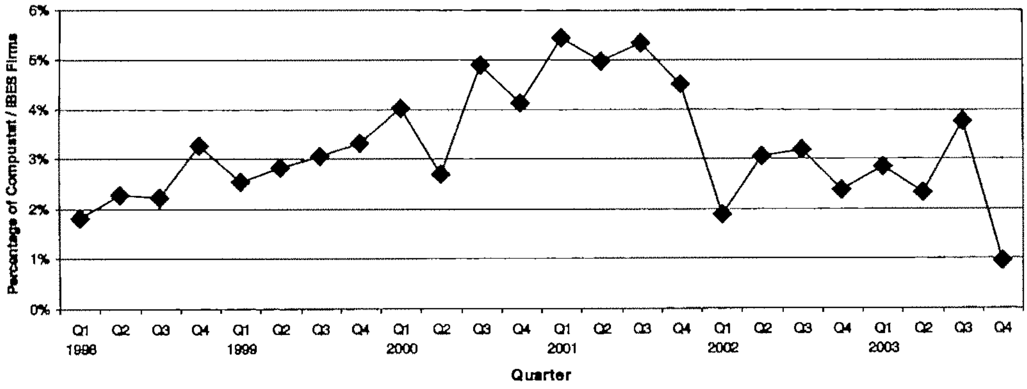
All Nomenclatures



Pro Forma Nomenclature



Other Nomenclatures



"other" nomenclature classification. While the use of the "pro forma" nomenclature dropped sharply after the passage of SOX, it appears to have recovered somewhat during 2003. Interestingly, the use of other nomenclatures dropped as well, but has not recovered to the extent that the pro forma nomenclature has recovered by the end of 2003.

We next investigate the types of firms that report adjusted-GAAP figures more frequently than others. Figure 3 illustrates the frequency of adjusted-GAAP reporting by nomenclature across industry classifications compared to the frequency distributions of the Compustat and I/B/E/S populations. The results suggest that companies that voluntarily disclose pro forma earnings are clustered in a few industries (i.e., they are not simply a random draw from the entire population of publicly traded firms). Figure 3 indicates that firms issuing alternative profitability measures using the "pro forma" nomenclature are heavily concentrated in certain manufacturing (SIC codes 3000–3999) and business service industries (SIC codes 7000–7999), while those using other nomenclatures are highly concentrated in financial service industries (SIC codes 6000–6999). Figure 3 depicts that these industry concentrations vary significantly from both the Compustat and I/B/E/S populations, whose distributions are similar to one another.

We next examine the frequency with which individual companies report adjusted-GAAP earnings figures during our six-year sample period and report the results in Figure 4. The figure reveals that firms announce adjusted-GAAP earnings numbers both infrequently and sporadically. Consistent with BBCL, Figure 4 indicates that 84 percent of our

FIGURE 3
Industry Classification of Adjusted-GAAP Firms Relative to the Compustat and I/B/E/S Populations

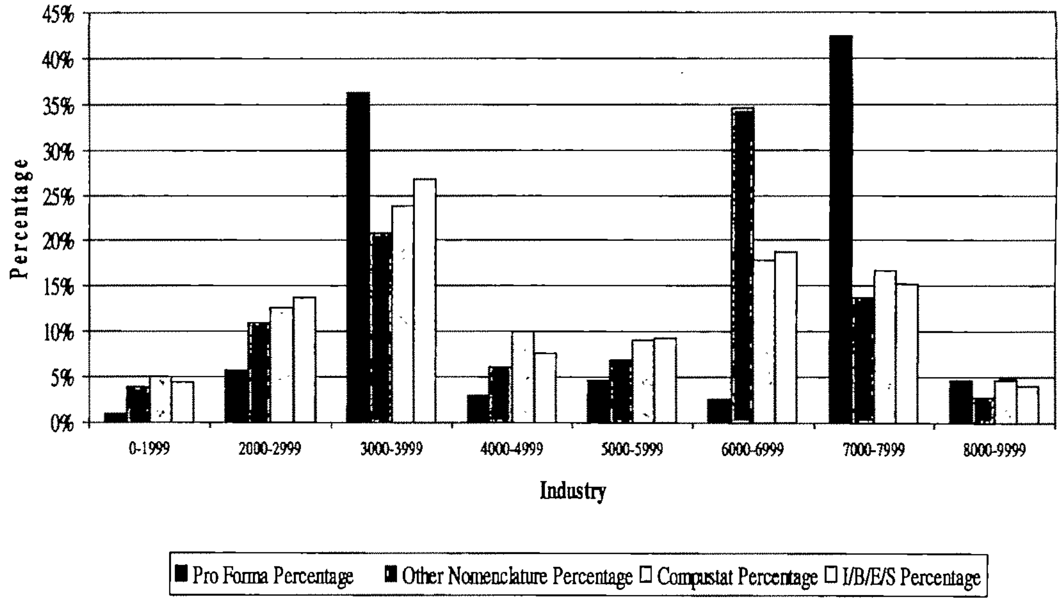
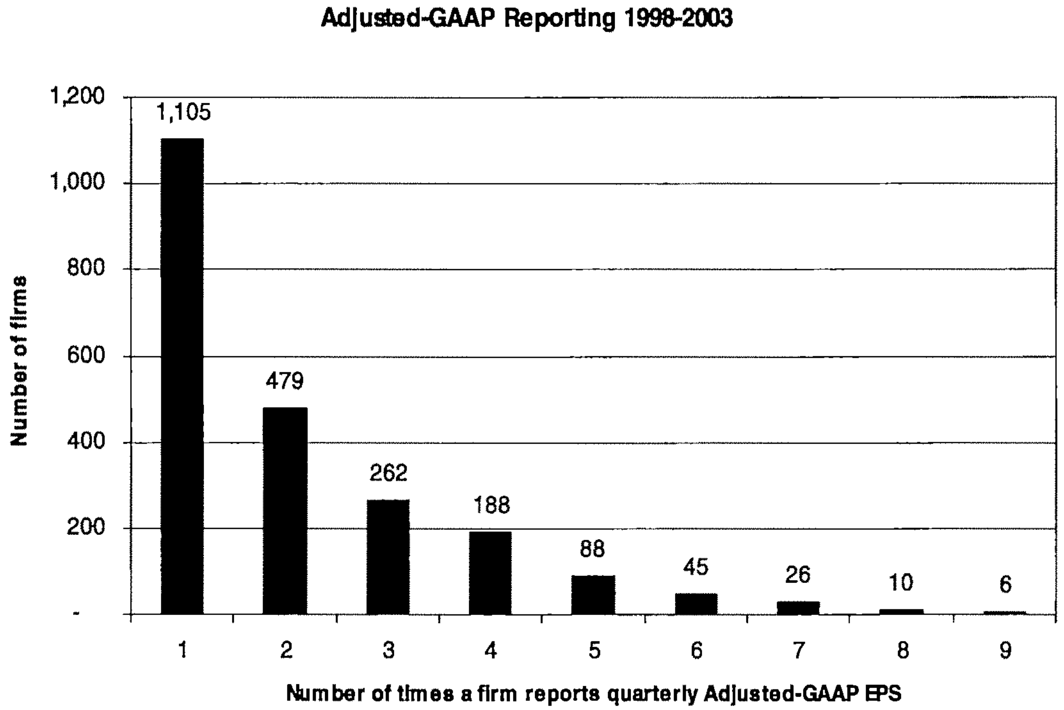


FIGURE 4
Frequency of Adjusted-GAAP Reporting during the Sample Period



sample firms [(1,105 + 479 + 262) ÷ 2,209] report a pro forma (adjusted-GAAP) earnings number three times or less during our entire 24-quarter sample period. This suggests a second level of self-selection. Not only is it a small subset of firms concentrated in particular industries that voluntarily reports adjusted-GAAP earnings, but these firms choose when to report these alternative profitability figures. Overall, our descriptive evidence suggests that research that employs I/B/E/S actual earnings figures (e.g., Bradshaw and Sloan 2002), or standard Compustat data items (e.g., Brown and Sivakumar 2003)—based on all or part of the I/B/E/S-Compustat population—as a proxy for manager-disclosed pro forma earnings, may not adequately capture the characteristics of the relatively small subset of firms whose managers voluntarily elect to report these numbers in select quarterly earnings press releases.[19,20]

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[19] While Bradshaw and Sloan (2002) use essentially the entire I/B/E/S-Compustat population, Brown and Sivakumar (2003) screen out observations where the I/B/E/S actual EPS number is equal to a Compustat operating EPS number.

[20] It is likely that our comprehensive search string does not capture all voluntarily disclosed adjusted-GAAP earnings figures. However, given Wallace's (2002) detailed categorization of common adjusted-GAAP nomenclatures, it is likely that our expanded search string identifies the majority of these reports. We still find that only 11 percent of the Compustat-I/B/E/S population reports these figures at the peak of adjusted-GAAP reporting. Therefore, we feel that the use of commercial database populations to proxy for management-issued adjusted-GAAP figures can largely obscure the unique characteristics of this select group of firms that voluntarily discloses pro forma earnings figures.

Descriptive Statistics

Table 1 provides descriptive statistics. Specifically, Panel A presents information about various firm characteristics, while Panel B provides descriptive statistics regarding the nature of announcement period market reactions. The median total assets of our sample firms is $621 million, while the mean is $6.2 billion—which is higher than the 75th percentile

TABLE 1

Comparison of Our Earnings Metrics to the Gu and Chen (2004) Framework

Variable[a]	25th Percentile	Mean	Median	75th Percentile
Panel A: Firm Characteristics, Earnings Metrics, and Forecast Errors				
SIZE	185.034	6,172.211	620.780	2,458.769
PRICE	8.680	25.088	17.562	31.500
$EPS_{GAAP\text{-}OP}$	−0.121	0.059	0.090	0.330
$EPS_{I/B/E/S}$	−0.030	0.187	0.130	0.350
$EPS_{PROFORMA}$	−0.010	0.223	0.150	0.390
$FE_{GAAP\text{-}OP}$	0.000	−0.009	0.000	0.000
$FE_{I/B/E/S}$	0.000	0.025	0.000	0.001
$FE_{PROFORMA}$	0.000	0.114	0.001	0.003
Panel B: Order Imbalance Variables				
SML_NETBUY_{-1}	−0.017	0.010	0.000	0.024
SML_NETBUY_{0}	−0.016	0.010	0.001	0.028
SML_NETBUY_{+1}	−0.013	0.019	0.003	0.025
MED_NETBUY_{-1}	−0.045	0.011	0.003	0.057
MED_NETBUY_{0}	−0.048	0.019	0.009	0.074
MED_NETBUY_{+1}	−0.037	0.013	0.006	0.054
LRG_NETBUY_{-1}	−0.111	−0.011	0.005	0.140
LRG_NETBUY_{0}	−0.130	−0.024	0.006	0.183
LRG_NETBUY_{+1}	−0.097	−0.021	0.005	0.123

[a] Statistics for all variables are based on 5,736 observations.

SIZE = total assets in $ millions at the end of the previous quarter;
PRICE = stock price five days before the pro forma earnings announcement date;
$EPS_{GAAP\text{-}OP}$ = Compustat diluted operating earnings per share;
$EPS_{PROFORMA}$ = managers' adjusted-GAAP earnings per share from the press release;
$EPS_{I/B/E/S}$ = I/B/E/S actual earnings per share;
$FE_{GAAP\text{-}OP}$ = forecast error calculated as Compustat diluted operating EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;
$FE_{PROFORMA}$ = forecast error calculated as managers' adjusted-GAAP EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;
$FE_{I/B/E/S}$ = forecast error calculated as I/B/E/S actual EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;
SML_NETBUY = abnormal net-buy volume (buy-volume less sell-volume) of small investors (all trades less than or equal to $7,000) on day *t* relative to the earnings announcement date scaled by non-announcement period total volume;
MED_NETBUY = abnormal net-buy volume (buy-volume less sell-volume) of medium-sized investors (all trades greater than $7,000 and less than or equal to $50,000) on day *t* relative to earnings announcement date scaled by non-announcement period total volume; and
LRG_NETBUY = abnormal net-buy volume (buy-volume less sell-volume) of large investors (all trades greater $50,000) on day *t* relative to the earnings announcement date scaled by non-announcement period total volume.

($2.5 billion). This suggests that while some extremely large firms voluntarily disclose adjusted-GAAP earnings numbers in their quarterly press releases, most pro forma firms are relatively small. The stock price distribution is also slightly positively skewed with a mean of $25.09 and a median of $17.56 per share. Panel A also presents descriptive statistics for the three earnings metrics. As mentioned earlier, our measure of GAAP diluted operating earnings per share, $EPS_{GAAP\text{-}OP}$, excludes both below-the-line items and special items. The mean of $EPS_{GAAP\text{-}OP}$ is $0.06, suggesting that the average operating earnings for our sample firms is positive.[21] The mean I/B/E/S actual earnings per share, $EPS_{I/B/E/S}$, is $0.19. The fact that $EPS_{I/B/E/S}$ is higher than $EPS_{GAAP\text{-}OP}$ suggests that analysts may exclude some recurring expenses, since the vast majority of one-time items (below-the-line and special items) are already excluded from our measure of GAAP operating earnings. Finally, the mean $EPS_{PROFORMA}$ is $0.22 suggesting that managers often exclude more nonrecurring expenses than do analysts.[22] Panel A also includes descriptive statistics for the three forecast error variables used in our analyses.[23] The mean $FE_{GAAP\text{-}OP}$, $-0.01, suggests that, on average, the GAAP operating income figure (which generally excludes all transitory items) falls just short of meeting analysts' forecasts. Consistent with the notion that analysts sometimes exclude recurring expenses (i.e., more than just below-the-line and special items) from their forecasts, the mean $FE_{I/B/E/S}$ is positive, $0.03. Finally, the results suggest that managers are more aggressive in excluding expenses than analysts since the mean $FE_{PROFORMA}$, $0.11, is even more positive and significantly greater than the mean forecast errors based on both GAAP operating EPS and I/B/E/S actual EPS. Interestingly, the Pearson correlations among the three forecast error measures are all significant, although none of the pair-wise correlations exceeds 30 percent.[24] This suggests that earnings surprise measures based on GAAP operating earnings, manager-disclosed pro forma earnings, and I/B/E/S actual EPS capture unique, non-overlapping information sets.

Panel B of Table 1 presents descriptive statistics for our abnormal net-buy measures. These variables represent the abnormal net-buy volume (buy volume less sell volume) for different trade-size groups. We observe above-normal net-buying activity by small and medium investors, while somewhat below-normal net-buying activity by large investors during the announcement period. We also find that small and medium abnormal net-buy measures are right-skewed as the means are greater than the medians, while the large abnormal net-buy measure is left-skewed.

Who Trades on Pro Forma Information?

Our first research question investigates *who* trades on pro forma information—sophisticated investors (e.g., institutions), less sophisticated individual investors, or both.

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[21] Our sample firms' average bottom-line GAAP EPS is $-0.04. Thus, firms that voluntary announce pro forma (adjusted-GAAP) earnings are, on average, unprofitable.

[22] We compare all three earnings metrics using both parametric t-tests and nonparametric Wilcoxon rank-sum tests. We find that central tendencies (mean and median) of $EPS_{GAAP\text{-}OP}$, $EPS_{I/B/E/S}$, and $EPS_{PROFORMA}$ are highly statistically different from each other. This illustrates that although the three earnings metrics have some commonality in terms of exclusions, there are also significant differences among them. This further bolsters our motivation for using all three earnings metrics in our examination of sophisticated versus less sophisticated investor reactions around pro forma announcement dates because each earnings metric may have a significant incremental contribution over the other two.

[23] In the spirit of conciseness, we only tabulate results based on forecast error variables that use the mean analyst forecast as the earnings expectation. Results based on the seasonal random-walk earnings expectation are slightly weaker since the seasonal random-walk is a much more noisy expectation than analysts' consensus forecast. However, the results are qualitatively similar and the study's main inferences remain unchanged.

[24] We obtain the following Pearson correlation coefficients among the three forecast error measures: $\rho(FE_{GAAP\text{-}OP}, FE_{I/B/E/S}) = 0.30$, $\rho(FE_{GAAP\text{-}OP}, FE_{PROFORMA}) = 0.29$, $\rho(FE_{PROFORMA}, FE_{I/B/E/S}) = 0.26$.

Our second research question examines the extent to which the net-buying activities of more and less sophisticated investors are associated with the pro forma forecast error vis-à-vis forecast errors based on GAAP operating EPS and I/B/E/S actual EPS. As previously mentioned, we investigate three trade-size categories: small, medium, and large. We expect that the small trade category will primarily capture the activities of less sophisticated and less informed individual investors, while the medium and large trade categories will primarily capture sophisticated institutional trading. We first examine less sophisticated investors' trading activities around pro forma earnings announcement dates by estimating the following four regression models:

$$SML\_NETBUY_{i,t} = \alpha_0 + \alpha_1 FE_{GAAP\text{-}OPi} + \alpha_4 SIZE_i + \alpha_5 MKTVOL_t + \varepsilon \quad (1)$$

$$SML\_NETBUY_{i,t} = \alpha_0 + \alpha_2 FE_{PROFORMAi} + \alpha_4 SIZE_i + \alpha_5 MKTVOL_t + \varepsilon \quad (2)$$

$$SML\_NETBUY_{i,t} = \alpha_0 + \alpha_3 FE_{I/B/E/Si} + \alpha_4 SIZE_i + \alpha_5 MKTVOL_t + \varepsilon \quad (3)$$

$$SML\_NETBUY_{i,t} = \alpha_0 + \alpha_1 FE_{GAAP\text{-}OPi} + \alpha_2 FE_{PROFORMAi} + \alpha_3 FE_{I/B/E/Si} + \alpha_4 SIZE_i + \alpha_5 MKTVOL_t + \varepsilon \quad (4)$$

where:

$SML\_NETBUY_{i,t}$ = small investors' day t *abnormal* net-buy volume;
$FE_{GAAP\text{-}OPi}$ = firm *i*'s *signed* GAAP operating earnings forecast error;
$FE_{PROFORMAi}$ = firm *i*'s *signed* pro forma forecast error;
$FE_{I/B/E/Si}$ = firm *i*'s *signed* I/B/E/S forecast error;
$SIZE_i$ = the log of firm *i*'s total assets at the end of the previous quarter;
$MKTVOL_t$ = market-wide trading volume on day *t*.

The first three models examine the extent to which each of the three earnings surprise metrics *separately* explains less sophisticated investors' abnormal net-buying activities during the three days in the announcement period (day −1, day 0 and day +1). A positive (negative) value for *SML_NETBUY* indicates above-normal (below-normal) net-buying activities by less sophisticated investors during the event period. The forecast errors in these analyses are also directional (signed). For example, a positive $FE_{PROFORMA}$ indicates good news based on pro forma earnings (i.e., the actual pro forma figure disclosed by managers is greater than the earnings expectation). Consequently, a significantly positive coefficient on $FE_{PROFORMA}$ in Model 2, α_2, would indicate that a higher (lower) pro forma forecast error results in higher (lower) *abnormal* net-buying activities by small investors. In other words, a positive α_2 indicates that less sophisticated investors are trading (net buying or selling) in the direction of the pro forma earnings surprise. Thus, the regression coefficients on the forecast errors in these models are interpreted in the same way one would interpret an earnings response coefficient (ERC) in a return-earnings regression (i.e., a positive ERC indicates that price moves in the same direction as the earnings surprise). Finally, Model 4 regresses *SML_NETBUY* simultaneously on all three forecast errors. Thus, Model 4 estimates the incremental explanatory power of each earnings forecast error, after controlling for the other two forecast errors. Since prior research suggests that smaller and larger firms have different investor clienteles (e.g., Bhushan 1989; El-Gazzar 1998; Lee and Radhakrishna 2000), we include *SIZE* to ensure that our results are not attributable to differences in firm size. Finally, *MKTVOL* controls for the influence of market-wide macroeconomic factors on investor trading responses.[25,26]

Table 2 reports the results of estimating Models 1 through 4. Model 1 results indicate that the coefficient on the GAAP operating earnings forecast error, $FE_{GAAP\text{-}OP}$ (α_1), is never significant on any day in the event window, suggesting that less sophisticated investors do not trade on GAAP operating earnings information. Model 2 results indicate that the coefficient on, $FE_{PROFORMA}$ (α_2), is positive and highly significant (two-tailed $p\text{-value} < 0.001$) on day +1 of the announcement window, suggesting that less sophisticated investors generally trade in the same direction as the pro forma forecast error (i.e., buy on pro forma good news and sell on pro forma bad news) the day *after* the earnings announcement. The coefficient on $FE_{PROFORMA}$, α_2, is not significant on day 0, but it is marginally significant and negative on day −1. This result is consistent with the notion that some less sophisticated investors anticipate the pro forma earnings news and trade in the direction opposite the pro forma forecast error the day before the announcement. However, since the coefficient is only marginally significant ($p\text{-value} = 0.09$), we are reluctant to draw conclusive inferences regarding this result. The results for Model 3 indicate that the coefficient on $FE_{I/B/E/S}$, α_3, is significantly positive on day 0 and day +1, suggesting that less sophisticated investors trade on information in I/B/E/S actual earnings on the day of and the day after the announcement. Overall, the results for Models 1 through 3 indicate that less sophisticated investors trade based on (1) information in pro forma earnings on day +1, and (2) information in I/B/E/S actual EPS figures on days 0 and +1, but do not trade on the GAAP operating earnings.[27,28]

Model 4 includes all three forecast errors simultaneously to allow us to examine the incremental significance of each in explaining less sophisticated investors' abnormal net-buy volume after controlling for the information in the other two earnings surprise variables. The results indicate that on day −1, the coefficient on $FE_{PROFORMA}$ (α_2) is marginally significantly negative, while the coefficients on $FE_{GAAP\text{-}OP}$ (α_1) and $FE_{I/B/E/S}$ (α_3) are insignificant. However, the F-tests indicate that the coefficient on $FE_{PROFORMA}$ is not significantly different from the (insignificant) coefficients on the other two forecast errors. The earnings announcement date (day 0) results displayed for Model 4 indicate that the coefficient on $FE_{GAAP\text{-}OP}$ (α_1) is insignificant and the coefficient on $FE_{PROFORMA}$ (α_2) is marginally negatively significant. Again, the F-test suggests that α_1 and α_2 are not significantly different from each other. The coefficient on $FE_{I/B/E/S}$ (α_3), on the other hand, is significantly positive

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[25] We winsorize all variables used in the regression analyses at the 1st and 99th percentiles to reduce the influence of extreme observations.

[26] To mitigate the effects of skewness in the data, we log-transform small, medium, and large abnormal net-buy measures, the forecast error variables, and the firm size measure in the regression models. To avoid taking the logarithm of negative or zero numbers, we add a positive constant whenever necessary (e.g., Ajinkya and Jain 1989; Richardson et al. 1986). We also repeat our main analyses using rank-transformed data and obtain qualitatively similar results. Finally we find that, despite skewness in the data, our inferences are unchanged when we use completely untransformed variables in our analyses. This suggests that our inferences are quite robust and not sensitive to any particular transformation.

[27] We note that the coefficient on *SIZE* is negative and significant on day +1 in all four models, suggesting that less sophisticated investors are more likely to buy stocks of smaller firms. The coefficient on *MKTVOL* is also positive and significant on day +1 for models 1 and 3, suggesting that market-wide factors influence less sophisticated investors' trading.

[28] Additional (untabulated) analyses suggest that the significant association between less sophisticated investors' abnormal net-buy volume and the pro forma earnings surprise for day +1 relative to the earnings announcement holds: (1) for all subperiods during 1998 to 2003, (2) for all nomenclatures used by managers to describe their pro forma earnings figures, and (3) whether the pro forma earnings figure or the GAAP number is emphasized (placed first) in the press release. Thus, our main inference that less sophisticated investors rely on information in pro forma earnings is quite robust.

TABLE 2

Small Investors' Abnormal Net-Buy Volume Regressed on the GAAP Operating Earnings Forecast Error, Pro Forma Forecast Error, I/B/E/S Forecast Error, and Control Variables

(Based on 5,736 Pro Forma Press Releases Issued between 1998 and 2003)[a]

Observation Interval	Intercept (α_0)	Coefficient on $FE_{GAAP\text{-}OPI}$ (α_1)	Coefficient on $FE_{PROFORMAI}$ (α_2)	Coefficient on $FE_{I/B/E/SI}$ (α_3)	Coefficient on $SIZE_i$ (α_4)	Coefficient on $MKTVOL_t$ (α_5)	Adjusted R^2	F-Test H_o: $\alpha_1 = \alpha_2$	F-Test H_o: $\alpha_1 = \alpha_3$	F-Test H_o: $\alpha_2 = \alpha_3$
Model 1: $SML\_NETBUY_{i,t} = \alpha_0 + \alpha_1 FE_{GAAP\text{-}OPI} + \alpha_4 SIZE_i + \alpha_5 MKTVOL_t + \varepsilon$										
Day −1	0.006 (0.65)	−0.021 (−0.73)			−0.001 (−1.00)	0.001 (0.49)	−0.02%			
Day 0	−0.002 (−0.19)	0.028 (0.77)			0.000 (−0.00)	0.002 (1.06)	−0.02%			
Day +1	0.054 (6.14)***	0.027 (0.99)			−0.008 (−10.60)***	0.002 (2.09)**	1.94%			
Model 2: $SML\_NETBUY_{i,t} = \alpha_0 + \alpha_2 FE_{PROFORMAI} + \alpha_4 SIZE_i + \alpha_5 MKTVOL_t + \varepsilon$										
Day −1	0.008 (0.81)		−0.045 (−1.70)*		−0.001 (−1.28)	0.001 (0.58)	0.02%			
Day 0	−0.002 (−0.13)		−0.028 (−0.83)		0.000 (−0.09)	0.002 (1.05)	−0.02%			
Day +1	0.048 (5.46)***		0.224 (9.27)***		−0.007 (−9.14)***	0.002 (1.59)	3.37%			

(continued on next page)

TABLE 2 (continued)

Model 3: $SML\_NETBUY_{i,t} = \alpha_0 + \alpha_3 FE_{I/B/E/Si} + \alpha_4 SIZE_i + \alpha_5 MKTVOL_t + \varepsilon$										
Day −1	0.006			0.069	−0.001	0.001	−0.02%			
	(0.64)			(0.62)	(−1.01)	(0.53)				
Day 0	−0.004			0.539	0.000	0.002	0.21%			
	(−0.31)			(3.74)***	(0.18)	(1.05)				
Day +1	0.052			0.600	−0.008	0.002	2.48%			
	(5.98)***			(5.73)***	(−10.34)***	(2.04)**				
Model 4: $SML\_NETBUY_{i,t} = \alpha_0 + \alpha_1 FE_{GAAP\text{-}OPi} + \alpha_2 FE_{PROFORMAi} + \alpha_3 FE_{I/B/E/Si} + \alpha_4 SIZE_i + \alpha_5 MKTVOL_t + \varepsilon$										
Day −1	0.007	−0.016	−0.049	0.143	−0.001	0.001	0.01%	0.50	1.46	2.24
	(0.79)	(−0.51)	(−1.75)*	(1.19)	(−1.22)	(0.55)				
Day 0	−0.002	0.004	−0.067	0.610	0.000	0.002	0.24%	1.36	13.02***	16.93***
	(−0.19)	(0.09)	(−1.85)*	(3.96)***	(−0.10)	(1.15)				
Day +1	0.046	−0.077	0.217	0.433	−0.007	0.002	3.65%	46.75***	17.60***	3.28*
	(5.30)***	(−2.66)***	(8.41)***	(3.89)***	(−8.82)***	(1.50)				

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).

[a] Parameter estimates reported first followed by t-statistics in parentheses.

$SML\_NETBUY_{i,t}$ = abnormal net-buy volume (buy-volume less sell-volume) of small investors (all trades less than or equal to \$7,000) on day t relative to the earnings announcement date;

$FE_{GAAP\text{-}OPi}$ = signed forecast error calculated as Compustat diluted operating EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;

$FE_{PROFORMAi}$ = signed forecast error calculated as managers' adjusted-GAAP EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;

$FE_{I/B/E/Si}$ = signed forecast error calculated as I/B/E/S actual EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;

$SIZE_i$ = the log of total assets in \$ millions at the end of the previous quarter; and

$MKTVOL_t$ = total market volume of all firms on CRSP on day t scaled by the total number of shares outstanding on day t.

and also significantly greater than the coefficients on $FE_{GAAP\text{-}OP}$ and $FE_{PROFORMA}$. This evidence suggests that the I/B/E/S forecast error has significant incremental explanatory power for small-investor abnormal net-buy volume over the other two forecast errors on the day of the announcement. On day +1, both the coefficients on $FE_{PROFORMA}$ (α_2) and $FE_{I/B/E/S}$ (α_3) are significantly positive, suggesting that each has significant incremental explanatory power relative to the other on the day after the announcement (F-statistics indicate that each coefficient is statistically significantly different from the other). The coefficient on $FE_{GAAP\text{-}OP}$ (α_1) is significantly negative and the F-tests indicate that both α_2 and α_3 are significantly greater than α_1.[29,30,31]

In summary, the results for Models 1 through 4 paint a consistent picture. We find that GAAP operating earnings is not a significant explanator of less sophisticated investors' abnormal net-buying activity during the announcement period. On the other hand, not only is the pro forma forecast error a significant explanator of less sophisticated investors' abnormal net-buying activities on day +1, but also it has significant incremental explanatory power relative to the forecast errors based on I/B/E/S and GAAP operating earnings on day +1. Finally, the forecast error based on I/B/E/S actual earnings is significantly incrementally associated with less sophisticated investors' abnormal net-buy volume on days 0 and +1.[32]

Table 3 reports the results of estimating Models 1 through 4 after replacing *SML_NETBUY* with *MED_NETBUY* as the dependent variable. *MED_NETBUY* is designed to capture the abnormal net-buying activities of sophisticated investors. Investors who submit medium-sized orders may include wealthy individuals or institutions intending to disguise their private information by breaking large orders into smaller trades (e.g., Cornell and Sirri 1992; Meulbroek 1992; Barclay and Warner 1993). The results from estimating Models 1 and 2 in Table 3 indicate that neither the operating GAAP forecast error nor the pro forma

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[29] One interpretation is that the GAAP operating earnings surprise has no incremental explanatory power after controlling for pro forma and I/B/E/S earnings surprises. The fact that the coefficient on the GAAP operating earnings forecast error is negative could be a mechanical result. For example, it is possible that additional exclusions of nonrecurring expenses can convert an operating earnings loss into a pro forma or I/B/E/S profit. Thus, if less sophisticated investors generally buy based on good pro forma or I/B/E/S news (i.e., additional exclusions of nonrecurring expenses that convert the GAAP operating loss to a profit), their trades will also appear to be systematically opposite in direction from the GAAP operating earnings forecast error. Therefore, it is conceivable that the negative coefficient on $FE_{GAAP\text{-}OP}$ is likely a mechanical result driven by the abnormal net-buying activities of less sophisticated investors spurred by good news based on $FE_{PROFORMA}$ and $FE_{I/B/E/S}$ on day +1.

[30] Since prior research finds that trading volume is significantly positively associated with the absolute price change (e.g., Karpoff 1987), we control for firm-specific abnormal returns in regression models 1 through 4 as an additional robustness check. This additional control ensures that our results are not attributable to the actions of investors who simply trade following large price changes. Consequently, we repeat our Table 2 analyses after including the daily size-adjusted abnormal return as an additional control variable. The abnormal return variable is never significant on days −1 and 0 and is always highly significantly positive on day +1, suggesting that less sophisticated investors do react significantly to large price changes. Nevertheless, our inferences are unchanged even after including this additional control, providing evidence that our results are not attributable to less sophisticated investors' response to large price movements.

[31] Collinearity diagnostics indicate that all condition indices are well within acceptable levels for all regression models (Belsley et al. 1980).

[32] Interestingly, when we estimate Models 1 through 4 using the seasonal random-walk forecast as the earnings expectation, we no longer find that the earnings surprise based on I/B/E/S actual EPS is significantly correlated with *SML_NETBUY* separately (Model 3) or together with the other earnings surprise variables (Model 4). In sharp contrast, even when we use the random-walk earnings expectation, the pro forma forecast error is still highly significantly positively associated with *SML_NETBUY* separately (Model 2) as well as jointly with the other forecast errors (Model 4). Therefore, the result that less sophisticated investors trade based on information in pro forma earnings is likely more robust than the result that these investors trade based on I/B/E/S actual earnings.

TABLE 3

Medium-Sized Investors' Abnormal Net-Buy Volume Regressed on the GAAP Operating Earnings Forecast Error, Pro Forma Forecast Error, I/B/E/S Forecast Error, and Control Variables

(Based on 5,736 Pro Forma Press Releases Issued between 1998 and 2003)[a]

Observation Interval	Intercept (β_0)	Coefficient on $FE_{GAAP\text{-}OPI}$ (β_1)	Coefficient on $FE_{PROFORMAI}$ (β_2)	Coefficient on $FE_{I/B/E/SI}$ (β_3)	Coefficient on $SIZE_i$ (β_4)	Coefficient on $MKTVOL_t$ (β_5)	Adjusted R^2	F-Test H_o: $\beta_1 = \beta_2$	F-Test H_o: $\beta_1 = \beta_3$	F-Test H_o: $\beta_2 = \beta_3$
Model 1: $MED\_NETBUY_{i,t} = \beta_0 + \beta_1 FE_{GAAP\text{-}OPI} + \beta_4 SIZE_i + \beta_5 MKTVOL_t + \varepsilon$										
Day −1	0.006	−0.071			0.000	0.000	−0.01%			
	(0.43)	(−1.57)			(0.20)	(0.19)				
Day 0	0.024	0.053			0.000	−0.001	−0.03%			
	(1.39)	(1.02)			(0.14)	(−0.51)				
Day +1	0.023	0.022			0.000	−0.001	−0.03%			
	(1.90)*	(0.61)			(−0.40)	(−0.89)				
Model 2: $MED\_NETBUY_{i,t} = \beta_0 + \beta_2 FE_{PROFORMAI} + \beta_4 SIZE_i + \beta_5 MKTVOL_t + \varepsilon$										
Day −1	0.009		−0.057		0.000	0.001	−0.02%			
	(0.58)		(−1.41)		(−0.09)	(0.30)				
Day 0	0.025		−0.019		0.000	−0.001	−0.04%			
	(1.41)		(−0.40)		(0.13)	(−0.55)				
Day +1	0.023		−0.021		0.000	−0.001	−0.03%			
	(1.92)*		(−0.65)		(−0.46)	(−0.88)				

(continued on next page)

TABLE 3 (continued)

Observation Interval	Intercept (β_0)	Coefficient on $FE_{GAAP\text{-}OPI}$ (β_1)	Coefficient on $FE_{PROFORMAI}$ (β_2)	Coefficient on $FE_{I/B/E/SI}$ (β_3)	Coefficient on $SIZE_i$ (β_4)	Coefficient on $MKTVOL_t$ (β_5)	Adjusted R^2	F-Test H_0: $\beta_1 = \beta_2$	F-Test H_0: $\beta_1 = \beta_3$	F-Test H_0: $\beta_2 = \beta_3$
Model 3: $MED\_NETBUY_{i,t} = \beta_0 + \beta_3 FE_{I/B/E/SI} + \beta_4 SIZE_i + \beta_5 MKTVOL_t + \varepsilon$										
Day −1	0.007			−0.090	0.000	0.000	−0.05%			
	(0.46)			(−0.51)	(0.10)	(0.27)				
Day 0	0.023			0.235	0.000	−0.001	−0.02%			
	(1.34)			(1.17)	(0.24)	(−0.56)				
Day +1	0.022			0.354	0.000	−0.001	0.07%			
	(1.81)*			(2.49)**	(−0.26)	(−0.93)				
Model 4: $MED\_NETBUY_{i,t} = \beta_0 + \beta_1 FE_{GAAP\text{-}OPI} + \beta_2 FE_{PROFORMAI} + \beta_3 FE_{I/B/E/SI} + \beta_4 SIZE_i + \beta_5 MKTVOL_t + \varepsilon$										
Day −1	0.008	−0.058	−0.043	0.027	0.000	0.000	−0.03%	0.04	0.17	0.12
	(0.52)	(−1.20)	(−0.98)	(0.14)	(0.04)	(0.23)				
Day 0	0.025	0.051	−0.047	0.228	0.000	−0.001	−0.03%	1.35	0.57	1.43
	(1.41)	(0.90)	(−0.93)	(1.06)	(0.04)	(−0.45)				
Day +1	0.023	0.007	−0.049	0.403	0.000	−0.001	0.07%	0.92	5.68**	7.69***
	(1.91)*	(0.19)	(−1.39)	(2.65)***	(−0.47)	(−0.84)				

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).

[a] Parameter estimates reported first followed by t-statistics in parentheses.

$MED\_NETBUY_{i,t}$ = abnormal net-buy volume (buy-volume less sell-volume) of medium-sized investors (all trades greater than $7,000 and less than or equal to $50,000) on day t relative to the earnings announcement date;
$FE_{GAAP\text{-}OPI}$ = signed forecast error calculated as Compustat diluted operating EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;
$FE_{PROFORMAI}$ = signed forecast error calculated as managers' adjusted-GAAP EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;
$FE_{I/B/E/SI}$ = signed forecast error calculated as I/B/E/S actual EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;
$SIZE_i$ = the log of total assets in $ millions at the end of the previous quarter; and
$MKTVOL_t$ = total market volume of all firms on CRSP on day t scaled by the total number of shares outstanding on day t.

forecast error are significantly associated with *MED_NETBUY* on any day in our announcement window. Model 3 indicates that the I/B/E/S forecast error is significantly positively associated with *MED_NETBUY* on day +1. When we include all three forecast errors in Model 4, we find similar results. The forecast errors based on GAAP operating income and pro forma earnings are not significant on any day in the event window, while the I/B/E/S forecast error is highly significant and positive on day +1. The F-tests indicate that the coefficient on the I/B/E/S forecast error is significantly greater than the coefficients on the other two forecast errors on day +1. Thus, medium-sized trades that likely capture sophisticated investors' stealth trading activities are associated with the I/B/E/S forecast error and not with the pro forma forecast error. This implies that, unlike the case of less sophisticated individual investors, sophisticated institutional investors base their trading decisions on information in I/B/E/S street earnings and not on information in managers' pro forma income figures.

Finally, we estimate Models 1 through 4 using *LRG_NETBUY* as the dependent variable. Table 4 reports these regression results for large orders. This table shows that none of the coefficients on any of the forecast errors is significant on any of the trading days in the announcement window. This suggests that sophisticated investors do not submit large orders based on the information in any of the earnings surprise variables during the three-day earnings announcement window. Table 1 shows that the *LRG_NETBUY* values are either negative or very close to zero. One of the implications of the combined results from Tables 1, 3, and 4 is that sophisticated investors generally avoid information-induced trading around pro forma announcement dates (i.e., their abnormal net-buying activities are either negative or zero). However, when they do trade, they likely engage in stealth trading by breaking their large orders into medium-sized trades and generally trade on information contained in the I/B/E/S earnings surprise.

Results from Tables 2 through 4 can be summarized as follows. First, less sophisticated investors trade on information in pro forma earnings and on information in I/B/E/S actual earnings. Second, earnings surprise variables (forecast errors) based on both pro forma and I/B/E/S street earnings have incremental explanatory power relative to the other for explaining less sophisticated investors' net-buying activities. Third, less sophisticated investors in pro forma firms do not trade on information in unexpected GAAP operating earnings. Finally, sophisticated investors either avoid trading around pro forma earnings announcements, or they trade based on information in I/B/E/S actual earnings using medium-sized trades, but do not trade at all based on manager-disclosed pro forma earnings information. Since this body of evidence suggests that less sophisticated investors trade on pro forma earnings information, while sophisticated investors do not, *if* some pro forma disclosures are indeed misleading, less sophisticated, individual investors are most at risk of being misled.[33]

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[33] One might argue that even though less sophisticated investors trade on pro forma information, while sophisticated investors do not, if the sophisticated professional investors almost always set the price, small investors may be "price takers" and thus be "price protected." Therefore, as an aside, we investigate the extent to which small, medium, and large abnormal net-buying activities are associated with announcement-period price movements. We regress size-adjusted abnormal returns, cumulated over the three-day announcement period window (CAR), on abnormal small, medium, and large net-buy measures (*SML_NETBUY, MED_NETBUY, LRG_NETBUY*) also cumulated over the three-day announcement window. We find that when CAR is regressed separately on each of the abnormal net-buy variables, each is significantly positively associated with CAR suggesting that the trading activities of all three investor groups are associated with announcement period price movements. However, Vuong's (1989) likelihood ratio tests reveal that the adjusted R^2 for the model where *SML_NETBUY* is the dependent variable is significantly higher than the adjusted R^2 values for the models where *MED_NETBUY*

(*continued on page 608*)

TABLE 4
Large Investors' Abnormal Net-Buy Volume Regressed on the GAAP Operating Earnings Forecast Error, Pro Forma Forecast Error, I/B/E/S Forecast Error, and Control Variables
(Based on 5,736 Pro Forma Press Releases Issued between 1998 and 2003)[a]

Observation Interval	Intercept (γ_0)	Coefficient on $FE_{GAAP\text{-}OPi}$ (γ_1)	Coefficient on $FE_{PROFORMAi}$ (γ_2)	Coefficient on $FE_{I/B/E/Si}$ (γ_3)	Coefficient on $SIZE_i$ (γ_4)	Coefficient on $MKTVOL_t$ (γ_5)	Adjusted R^2	F-Test H_o: $\gamma_1 = \gamma_2$	F-Test H_o: $\gamma_1 = \gamma_3$	F-Test H_o: $\gamma_2 = \gamma_3$
Model 1: $LRG\_NETBUY_{i,t} = \gamma_0 + \gamma_1 FE_{GAAP\text{-}OPi} + \gamma_4 SIZE_i + \gamma_5 MKTVOL_t + \varepsilon$										
Day −1	−0.027	0.070			0.007	−0.002	0.04%			
	(−0.71)	(0.62)			(2.09)**	(−0.41)				
Day 0	−0.147	0.199			0.015	0.006	0.29%			
	(−3.13)***	(1.42)			(3.97)***	(1.11)				
Day +1	0.029	−0.002			0.001	−0.002	−0.04%			
	(1.18)	(−0.03)			(0.39)	(−0.77)				
Model 2: $LRG\_NETBUY_{i,t} = \gamma_0 + \gamma_2 FE_{PROFORMAi} + \gamma_4 SIZE_i + \gamma_5 MKTVOL_t + \varepsilon$										
Day −1	−0.031		0.124		0.007	−0.002	0.05%			
	(−0.83)		(1.19)		(2.27)**	(−0.47)				
Day 0	−0.149		0.044		0.016	0.006	0.25%			
	(−3.15)***		(0.35)		(4.06)***	(1.01)				
Day +1	0.030		−0.043		0.001	−0.002	−0.03%			
	(1.22)		(−0.63)		(0.29)	(−0.74)				

(continued on next page)

TABLE 4 (continued)

Model 3: $LRG\_NETBUY_{i,t} = \gamma_0 + \gamma_3 FE_{I/B/E/Si} + \gamma_4 SIZE_i + \gamma_5 MKTVOL_t + \varepsilon$									
Day −1	−0.027 (−0.73)			0.084 (0.19)	0.007 (2.13)**	−0.002 (−0.44)	0.03%		
Day 0	−0.148 (−3.14)***			0.095 (0.17)	0.016 (4.06)***	0.006 (1.03)	0.25%		
Day +1	0.029 (1.16)			0.174 (0.59)	0.001 (0.41)	−0.002 (−0.78)	−0.03%		
Model 4: $LRG\_NETBUY_{i,t} = \gamma_0 + \gamma_1 FE_{GAAP\text{-}OPi} + \gamma_2 FE_{PROFORMAi} + \gamma_3 FE_{I/B/E/Si} + \gamma_4 SIZE_i + \gamma_5 MKTVOL_t + \varepsilon$									
Day −1	−0.031 (−0.81)	0.038 (0.30)	0.119 (1.07)	−0.094 (−0.20)	0.007 (2.23)**	−0.002 (−0.45)	0.02%	0.19	0.06	0.18
Day 0	−0.147 (−3.11)***	0.213 (1.40)	−0.006 (−0.05)	−0.149 (−0.26)	0.015 (3.89)***	0.006 (1.11)	0.25%	0.93	0.32	0.05
Day +1	0.030 (1.21)	−0.002 (−0.02)	−0.058 (−0.79)	0.245 (0.77)	0.001 (0.29)	−0.002 (−0.73)	−0.06%	0.21	0.51	0.79

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).

[a] Parameter estimates reported first followed by t-statistics in parentheses.

$LRG\_NETBUY_{i,t}$ = abnormal net-buy volume (buy-volume less sell-volume) of large investors (all trades greater $50,000) on day t relative to the earnings announcement date;
$FE_{GAAP\text{-}OPi}$ = signed forecast error calculated as Compustat diluted operating EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;
$FE_{PROFORMAi}$ = signed forecast error calculated as managers' adjusted-GAAP EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;
$FE_{I/B/E/Si}$ = signed forecast error calculated as I/B/E/S actual EPS minus the I/B/E/S mean forecast, scaled by price on day $t-5$;
$SIZE_i$ = the log of total assets in $ millions at the end of the previous quarter; and
$MKTVOL_t$ = total market volume of all firms on CRSP on day t scaled by the total number of shares outstanding on day t.

Sensitivity Analyses

Matched-Sample Tests

In order to further investigate the seemingly "lukewarm" trading responses of sophisticated investors around earnings announcements containing management-issued adjusted-GAAP figures, we investigate a matched sample of earnings announcements of firms that do not voluntarily disclose an alternative profitability figure in their quarterly earnings press releases. We collect a match for each firm-quarter observation in our sample by selecting a non-pro-forma "matched" firm for each of our pro forma firms based on firm size and industry.[34] We investigate small-, medium-, and large-investor abnormal net-buying activities around these (non-pro-forma) matched-sample earnings announcements.

The untabulated results indicate that the forecast error based on GAAP operating earnings is significantly positively associated with *MED_NETBUY*, but only on the day of the earnings announcement. More importantly, the results also reveal that the I/B/E/S forecast error is significantly positively associated with both *MED_NETBUY* as well as *LRG_NETBUY* on all three days in the announcement window. These results suggest that sophisticated investors generally trade significantly around earnings announcements. Recall that *MED_NETBUY* is never associated with GAAP operating earnings surprise in our sample of adjusted-GAAP earnings announcements, and it is only associated with the I/B/E/S earnings surprise on day +1 relative to the announcement, while *LRG_NETBUY* is never associated with GAAP operating earnings or I/B/E/S actual earnings surprise. Thus, the lukewarm trading reaction of sophisticated investors we observe around pro forma earnings announcements is not attributable to the lack of power in our sophisticated investor proxies. Rather it appears that when firms announce adjusted-GAAP earnings numbers, sophisticated investors either refrain from actively trading during the announcement period, or trade cautiously later in the announcement window primarily using medium-sized trades.

Finally, the matched-sample results indicate that *SML_NETBUY* is significantly associated with forecast errors based on both GAAP operating income and I/B/E/S street earnings on the day of the earnings announcement. However, neither earnings surprise variable is incrementally informative relative to the other in explaining less sophisticated investors' net-buying activities. Interestingly, we never find any evidence in our pro forma sample that less sophisticated investors trade on information in GAAP operating earnings,

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Footnote 33, continued
or *LRG_NETBUY* are the dependent variable. Moreover, when we regress CAR simultaneously on all three aggregated abnormal net-buy measures, the coefficient on *SML_NETBUY* is significantly higher (based on F-tests) than the coefficients on the other two abnormal net-buy measures, although all three coefficients are statistically significant. These results suggest that small investors may have a role in setting prices (which is not surprising given our evidence that sophisticated investors do not engage in particularly heavy trading around pro forma announcements), and as a result may not always be "price protected."

[34] For each of our pro forma firms, we first select all firms in the same Compustat size-decile (based on both market value of common equity and total assets) that are not part of our sample of pro forma announcers. From this pool, we then sequentially match on four-digit, three-digit, and two-digit SIC codes. If we obtain multiple matches based on this process within four-, three-, or two-digit SIC codes for a given pro forma firm, we randomly select one of the available matches. In all instances, we use the match from the most specific industry code with available data for our regression analyses. Given that our comprehensive search string likely picks up the majority of adjusted-GAAP announcements during our sample period, our matched-firms, by and large, did not announce adjusted-GAAP earnings figures along with their audited GAAP earnings numbers in their earnings press releases. However, to the extent that our search string could miss actual adjusted-GAAP earnings numbers in matched-sample press releases, it would work against our finding differences in market reactions between our pro forma sample and matched-sample firms.

implying that when presented with both pro forma and GAAP operating figures, less sophisticated investors focus on manager-adjusted pro forma information.[35]

An Alternative Specification to Assess the Sensitivity of the Results to the Errors-in-Variables Problem

Prior research discusses the potential ramifications of the classic errors-in-variables problem associated with using the mean analyst forecast as the expectation in calculating forecast errors for different earnings metrics (e.g., Bradshaw 2003; BBCL; Berger 2005). Since forecast tracking services, such as I/B/E/S, attempt to exclude the same items from the reported "street" earnings number that analysts exclude from their forecasts, the "street" forecast error will generally have less measurement error than forecast errors that match GAAP operating earnings or manager-adjusted pro forma earnings with analysts' expectation. In order to ensure that our results are not attributable to measurement error, we first use a random-walk earnings expectation based on GAAP operating earnings (as previously explained), and find qualitatively similar, though weaker results. Since the random-walk earnings expectation is a relatively outdated and inaccurate proxy for the market's expectation, the resulting earnings surprise measures are likely quite noisy (although the GAAP operating earnings forecast error based on this expectation is unbiased) and may reduce the power of the statistical tests. Therefore, we revisit the measurement error issue using a different approach. Gu and Chen (2004, Figure 1) provide a useful framework for understanding how standard GAAP earnings measures differ from the street earnings numbers published by forecast tracking services (such as I/B/E/S). We extend this framework to illustrate how our three earnings metrics differ from one another and to motivate alternative specifications of our regression models that are not sensitive to the errors-in-variables problem.

We first briefly discuss the Gu and Chen (2004) (hereafter, GC) framework and then introduce our alternative model specifications. Figure 5 illustrates the main elements of the GC framework.[36] GC separate what they call "Core EPS" (which is essentially recurring income), from the transitory components of earnings (special items and below-the-line items). *CORE_COMMON* in the first chart of Figure 5 represents recurring items that are included in both GAAP operating earnings as well as analysts' definition of street earnings. Analysts generally exclude nonrecurring special items from their street earnings number. We label special items excluded by analysts as *SPEC_ANALEXC*. GC also entertain the

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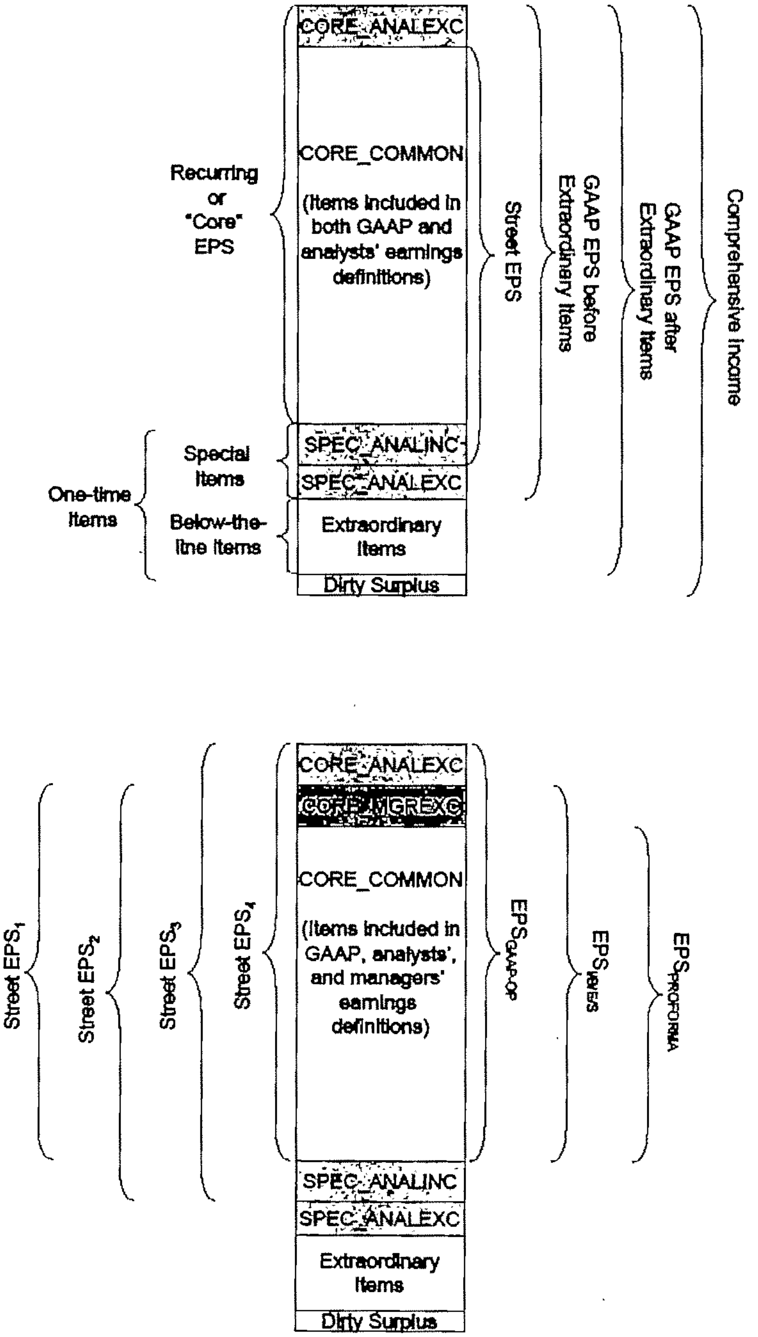
[35] We also perform alternative matched-sample analyses using a "within-sample" design in which we examine earnings announcements of our pro forma sample firms for quarters in which they do not disclose an adjusted-GAAP number (as defined by our comprehensive search string). We identify a "non-pro forma" quarter for each of our sample observations by systematically going back four quarters and then forward four quarters from the pro forma announcement date until we find a quarter where managers do not disclose an adjusted-GAAP number (i.e., the earnings announcement is not captured by our search strings). We then repeat our main analyses on this set of "non-pro forma" earnings announcements. We find that small investors trade on $FE_{GAAP\text{-}OP}$ on days 0 and +1 and on $FE_{I/B/E/S}$ on all three days during the announcement period. Medium investors also trade on $FE_{GAAP\text{-}OP}$ on day 0 and on $FE_{I/B/E/S}$ on all three days in the announcement window. However, we find no evidence that large investors trade on any of the forecast errors. Thus, sophisticated investors appear to use medium-sized trades in response to the GAAP operating earnings surprise on the day of the announcement and in response to the I/B/E/S actual earnings surprise throughout the announcement window. We note that when the same set of firms announce adjusted-GAAP earnings figures along with their standard GAAP earnings number, we find no evidence that either small or medium investors ever trade on GAAP operating earnings and medium investors only trade on I/B/E/S information on day +1. These analyses provide further support to the notion that sophisticated investors are skeptical about management-issued adjusted-GAAP profitability figures and trade more hesitantly when earnings announcements contain these disclosures.

[36] We use slightly different labels that we feel are more descriptive of the various earnings components mentioned in the GC framework.

FIGURE 5
Comparison of Our Earnings Metrics to the Gu and Chen (2004) Framework



possibility that analysts choose to include some one-time special items in their definition of street earnings (*SPEC_ANALINC*). However, it is plausible that analysts choose to exclude some recurring items from street earnings that are part of core EPS (*CORE_ANALEXC*).[37] The brackets on the right side of the first chart of Figure 5 illustrate how different EPS measures are defined in the context of GC's framework: (1) GC's characterization of street earnings, (2) GAAP EPS before extraordinary items, (3) GAAP EPS after extraordinary items, and (4) comprehensive income.

Managers' definition of adjusted-GAAP earnings is usually similar to analysts' definition of street earnings except that managers frequently exclude additional core or recurring items that analysts do not exclude. In fact, Table 1 shows that $EPS_{PROFORMA}$ is significantly greater than $EPS_{I/B/E/S}$ implying that managers exclude expenses more aggressively than do analysts. Thus, the second chart of Figure 5 includes a separate adjustment category for additional manager exclusions of recurring items (CORE_*MGREXC*) beyond those made by analysts (*CORE_ANALEXC*).[38] The left side of the second chart illustrates the fact that analysts' street earnings can have several alternative definitions. *Street* EPS_1 illustrates a situation where analysts exclude all one-time items as well as at least one recurring item. *Street* EPS_2 represents a situation where analysts choose to include one or more special items (*SPEC_ANALINC*) and they exclude at least one recurring item (*CORE_ANALEXC*). *Street* EPS_3 includes one or more one-time items (*SPEC_ANALINC*), but does not exclude any recurring items. Finally, *Street* EPS_4 represents a situation where analysts exclude all one-time items and do not exclude any recurring items.

The brackets on the right side of the second chart depict our three EPS metrics. Note that our definition of GAAP operating EPS, $EPS_{GAAP\text{-}OP}$, always excludes special items and "below-the-line" items. Thus, $EPS_{GAAP\text{-}OP}$ equals recurring EPS or GC's "Core EPS" in this framework. GC focus on how analysts' treatment of special items affects investors' perceptions of these items. They find that special items that analysts choose to include in their street earnings number (*SPEC_ANALINC*) are perceived by investors to be more persistent than special items that analysts choose to exclude from street earnings (*SPEC_ANALEXC*). While this result is interesting, both analysts and managers exclude one-time items the vast majority of the time. Thus, the applicability of GC's results is limited to somewhat rare situations where analysts choose to *include* a one-time item. Consequently, we focus on the more general scenario where analysts (and managers) choose to *exclude* all one-time items. Therefore, although we acknowledge that some of our $EPS_{I/B/E/S}$ observations correspond to the *Street* EPS_2, *Street* EPS_3, or *Street* EPS_4 definitions, the vast majority of our $EPS_{I/B/E/S}$ observations map into *Street* EPS_1. Finally, as mentioned earlier, managers generally exclude all one-time items (Extraordinary Items, *SPEC_ANALEXC*, and

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[37] One noticeable difference between our characterization of the GC framework in Figure 5 and the original Gu and Chen (2004) Figure 1 is the placement of analysts' "other exclusions" (which we call *CORE_ANALEXC*). Gu and Chen (2004) place analysts' additional exclusions between excluded one-time special items (which we call *SPEC_ANALEXC*) and extraordinary items in their Figure 1. However, we emphasize that one-time items are classified as "special items" or "below-the-line" extraordinary items for the vast majority of our observations. Thus, when analysts have "other exclusions," these exclusions are generally components of recurring income. Thus, we position other analyst exclusions (*CORE_ANALEXC*) in the core EPS section of the income statement and place this variable at the top of the "bar" in order to more easily isolate the components of different EPS measures.

[38] We find that analysts' mean (median) exclusion of recurring items (*CORE_ANALEXC*), calculated as $EPS_{I/B/E/S} - EPS_{GAAP\text{-}OP}$, is \$0.07 (\$0.01) and that managers' mean (median) *incremental* exclusion of recurring items (*CORE_MGREXC*), calculated as $EPS_{PROFORMA} - EPS_{I/B/E/S}$, is \$0.10 (\$0.00).

$SPEC\_ANALINC$) as well as additional recurring items beyond analysts' recurring exclusions. Thus, $EPS_{PROFORMA}$ simply represents $CORE\_COMMON$ in the second chart of Figure 5.

The advantage of the GC framework is that it allows us to investigate the extent to which investors focus on managers' incremental exclusions of recurring items ($CORE\_MGREXC$) beyond those excluded by analysts. Recall that in this framework, $CORE\_MGREXC$ is simply the difference between $EPS_{PROFORMA}$ and $EPS_{I/B/E/S}$. Specifically, if a particular investor group trades on managers' incremental adjustments, we would expect $CORE\_MGREXC$ to be significantly positively associated with that group's abnormal trading response. Hence, we use an alternative specification of Model 3 that allows us to investigate the extent to which sophisticated vis-à-vis less sophisticated investors react to managers' incremental adjustments:

$$SML\_NETBUY_{i,t} = \alpha_0 + \alpha_3 FE_{I/B/E/Si} + \alpha_4 CORE\_MGREXC + \alpha_5 SIZE_i + \alpha_6 MKTVOL_t + \varepsilon \tag{5a}$$

$$MED\_NETBUY_{i,t} = \beta_0 + \beta_3 FE_{I/B/E/Si} + \beta_4 CORE\_MGREXC + \beta_5 SIZE_i + \beta_6 MKTVOL_t + \varepsilon \tag{5b}$$

$$LRG\_NETBUY_{i,t} = \gamma_0 + \gamma_3 FE_{I/B/E/Si} + \gamma_4 CORE\_MGREXC + \gamma_5 SIZE_i + \gamma_6 MKTVOL_t + \varepsilon \tag{5c}$$

where $CORE\_MGREXC = EPS_{PROFORMA} - EPS_{I/B/E/S}$.

We employ $FE_{I/B/E/S}$ in these regressions to control for the earnings surprise since the (unbiased) I/B/E/S forecast error is the most accurate measure of surprise or "new information" in earnings. Specifically, this specification avoids the errors-in-variables problem because it does not match GAAP operating earnings or managers' pro forma earnings with analysts' consensus expectation to calculate the forecast error.

Thus, α_4, β_4, and γ_4 capture the incremental contribution of managers' additional exclusions of recurring items (beyond analysts' core exclusions) in explaining small, medium and large investors' abnormal net-buying activities, respectively. Since $CORE\_MGREXC$ equals $EPS_{PROFORMA}$ minus $EPS_{I/B/E/S}$, it is positive when $EPS_{PROFORMA}$ is greater than $EPS_{I/B/E/S}$. Consequently, a positive and significant α_4 would indicate that managers' incremental income-increasing exclusions of recurring items prompt small investors to intensify their abnormal net-buying activities, an indication that small investors trade based on managers' pro forma adjustments. Likewise, medium (large) investors' increased abnormal net-buying activities in response to managers' incremental income-increasing exclusions would result in a significantly positive β_4 (γ_4) coefficient.

Table 5 reports the results of estimating these regressions. The first panel reports Model 5a results and indicates that the coefficient on $FE_{I/B/E/S}$, α_3, is significantly positive on day 0 while the coefficient on $CORE\_MGREXC$, α_4, is significantly negative. Thus, it appears that a subset of small investors decreases their abnormal net-buying activities on the day of the announcement when managers voluntarily disclose income-increasing adjusted-GAAP earnings figures. In contrast, α_4 is highly significantly positive on day +1 (as is α_3). Thus, small investors appear to intensify their abnormal net-buying activities the day after

TABLE 5
The Impact of Managers' Incremental Adjustments beyond Analysts' Exclusions on Small, Medium, and Large Investor Abnormal Net-Buy Volume after Controlling for the I/B/E/S Forecast Error
(Based on 5,736 Pro Forma Press Releases Issued between 1998 and 2003)[a]

Model 5a: $SML\_NETBUY_{i,t} = \alpha_0 + \alpha_3 FE_{I/B/E/Si} + \alpha_4 CORE\_MGREXC + \alpha_5 SIZE_i + \alpha_6 MKTVOL_t + \varepsilon$

Observation Interval	Intercept (α_0)	Coefficient on $FE_{I/B/E/Si}$ (α_3)	Coefficient on $CORE\_MGREXC_i$ (α_4)	Coefficient on $SIZE_i$ (α_5)	Coefficient on $MKTVOL_t$ (α_6)	Adjusted R²
Day −1	0.007 (0.77)	0.068 (0.61)	−0.041 (−1.48)	−0.001 (−1.21)	0.001 (0.57)	−0.00%
Day 0	−0.002 (−0.19)	0.538 (3.74)***	−0.074 (−2.07)**	−0.000 (−0.11)	0.002 (1.16)	0.27%
Day +1	0.048 (5.45)***	0.591 (5.67)***	0.194 (7.55)***	−0.007 (−9.23)***	0.002 (1.68)*	3.43%

Model 5b: $MED\_NETBUY_{i,t} = \beta_0 + \beta_3 FE_{I/B/E/Si} + \beta_4 CORE\_MGREXC + \beta_5 SIZE_i + \beta_6 MKTVOL_t + \varepsilon$

Observation Interval	Intercept (β_0)	Coefficient on $FE_{I/B/E/Si}$ (β_3)	Coefficient on $CORE\_MGREXC_i$ (β_4)	Coefficient on $SIZE_i$ (β_5)	Coefficient on $MKTVOL_t$ (β_6)	Adjusted R²
Day −1	0.008 (0.56)	−0.091 (−0.52)	−0.053 (−1.21)	0.000 (−0.07)	0.001 (0.30)	−0.04%
Day 0	0.025 (1.42)	0.234 (1.16)	−0.064 (−1.28)	0.000 (0.06)	−0.001 (−0.49)	−0.01%
Day +1	0.023 (1.93)*	0.357 (2.51)**	−0.056 (−1.61)	−0.005 (−0.48)	−0.001 (−0.85)	0.10%

(continued on next page)

TABLE 5 (continued)

Model 5c: $LRG\_NETBUY_{i,t} = \gamma_0 + \gamma_3 FE_{I/B/E/Si} + \gamma_4 CORE\_MGREXC + \gamma_5 SIZE_i + \gamma_6 MKTVOL_t + \varepsilon$

Observation Interval	Intercept (γ_0)	Coefficient on $FE_{I/B/E/Si}$ (γ_3)	Coefficient on $CORE\_MGREXC_i$ (γ_4)	Coefficient on $SIZE_i$ (γ_5)	Coefficient on $MKTVOL_t$ (γ_6)	Adjusted R²
Day −1	−0.032	0.087	0.163	0.007	−0.002	0.05%
	(−0.85)	(0.20)	(1.48)	(2.31)**	(−0.49)	
Day 0	−0.148	0.095	0.005	0.016	0.006	0.24%
	(−3.13)***	(0.17)	(0.04)	(4.02)***	(1.03)	
Day +1	0.030	0.177	−0.062	0.001	−0.002	−0.04%
	(1.22)	(0.60)	(−0.85)	(0.29)	(−0.73)	

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively (two-tailed).

[a] Parameter estimates reported first followed by t-statistics in parentheses.

$SML\_NETBUY_{i,t}$ = abnormal net-buy volume (buy-volume less sell-volume) of small investors (all trades less than or equal to \$7,000) on day *t* relative to the earnings announcement date;
$MED\_NETBUY_{i,t}$ = abnormal net-buy volume (buy-volume less sell-volume) of medium-sized investors (all trades greater than \$7,000 and less than or equal to \$50,000) on day *t* relative to the earnings announcement date;
$LRG\_NETBUY_{i,t}$ = abnormal net-buy volume (buy-volume less sell-volume) of large investors (all trades greater \$50,000) on day *t* relative to the earnings announcement date;
$FE_{I/B/E/Si}$ = signed forecast error calculated as I/B/E/S actual EPS minus the I/B/E/S mean forecast, scaled by price on day *t*−5;
$CORE\_MGREXC_i$ = managers' incremental adjustments to earnings beyond those made by analysts (calculated as $EPS_{PROFORMA} - EPS_{I/B/E/S}$);
$SIZE_i$ = the log of total assets in \$ millions at the end of the previous quarter; and
$MKTVOL_t$ = total market volume of all firms on CRSP on day *t* scaled by the total number of shares outstanding on day *t*.

the earnings announcement in response to managers' income-increasing exclusions of recurring items, suggesting that managers' adjusted-GAAP disclosures influence small investors' trading decisions. The second panel reports results for Model 5b (medium orders) and reveals that while the coefficient on $FE_{I/B/E/S}$, β_3, is significant on day +1, the coefficient on *CORE_MGREXC*, β_4, is never statistically significant, suggesting that medium-sized investors do not buy incrementally more based on managers' incremental exclusions. Finally, the third panel repeats the same regressions for large orders (Model 5c) and shows that neither γ_3 nor γ_4 is ever significant, suggesting that large investors' trading activities are not associated with the forecast error or manager's exclusions during the announcement period.

Taken together, these analyses are highly consistent with our main results. We find that managers' income-increasing exclusions of recurring items beyond analysts' exclusions significantly increase abnormal net-buying activities of less sophisticated investors (i.e., the small trade-size group), but have no impact on the abnormal net-buying activities of sophisticated investors (i.e., the medium and large trade-size groups). Since this specification circumvents the errors-in-variables problem, our main inference—that primarily less sophisticated investors trade on information in pro forma earnings, while more sophisticated investors do not trade on pro forma earnings information—is robust to concerns about measurement error.

VI. CONCLUSION

Regulators and standard-setters have expressed concern that managers' pro forma disclosures are incomplete, inaccurate, and misleading to investors. Recent experimental research suggests that more versus less sophisticated investors may respond differently to these nonstandard, adjusted-GAAP earnings measures disclosed by managers. Since less wealthy, individual investors lack the necessary sophistication to understand the accuracy and reliability of these disclosures, they are most at risk of being misled. Consequently, this study examines intraday transactions around 5,736 earnings announcements that accompany pro forma disclosures between January 1998 and December 2003 to investigate which class of investors primarily trades on pro forma earnings information: sophisticated institutional investors, less wealthy and less sophisticated individual investors, or both.

Our results suggest that the earnings surprise based on pro forma earnings is significantly positively associated with the abnormal net-buying activities of the less sophisticated, primarily individual class of investors on the day after the announcement. The results also indicate that the pro forma earnings surprise is significantly incrementally informative relative to the earnings surprise measures based on GAAP operating earnings and I/B/E/S actual earnings in explaining less sophisticated investors' announcement-period abnormal net-buying activities. We find that less sophisticated investors also trade based on the I/B/E/S earnings surprise, which is incrementally informative relative to the pro forma and GAAP earnings surprises. In sharp contrast, we find that sophisticated investors either avoid trading around pro forma earnings announcements, or they trade later in the announcement period (only on day +1) based on information in I/B/E/S actual earnings, but they never trade based on manager-disclosed pro forma earnings information. In order to further investigate the seemingly "lukewarm" trading responses of sophisticated investors around earnings announcements containing management-issued pro forma figures, we investigate a matched sample of earnings announcements of firms that do not voluntarily disclose pro forma numbers. This additional analysis suggests that the lukewarm trading reaction of sophisticated investors is not attributable to the lack of power in our sophisticated investor proxies because these investors appear to trade significantly around the earnings announcements of the matched sample. Thus, it appears that when firms announce adjusted-GAAP earnings numbers, sophisticated investors either refrain from actively trading during the announcement period, or trade cautiously later in the announcement window. Overall, our results suggest that the market reaction to pro forma earnings information is almost exclusively attributable to the less sophisticated investors.

A caveat in our research design is that it is susceptible to measurement errors introduced by the errors-in-variables problem. This problem arises because forecast tracking services, such as I/B/E/S, attempt to exclude the same items from their "street" earnings number that analysts exclude from their forecasts. Thus, the I/B/E/S forecast error generally has less measurement error than forecast errors computed by subtracting the mean analysts' forecast from GAAP operating earnings or manager-adjusted pro forma earnings. In order to ensure that our results are not attributable to measurement error, we extend the Gu and Chen (2004) framework to motivate an alternative specification that allows us to examine investors' reactions to managers' incremental earnings adjustments beyond those made by analysts after controlling for earnings surprise. We subtract analysts' mean forecast from I/B/E/S actual EPS figure to measure earnings surprise. Since this earnings surprise measure does not suffer from the measurement error introduced by mismatching different definitions of earnings, this specification likely circumvents the errors-in-variables problem. Results of estimating this alternative specification indicate that less sophisticated investors' abnormal net-buying activities are significantly positively associated with the magnitude and direction of managers' incremental adjustments beyond analysts' adjustments even after appropriately controlling for earnings surprise. However, managers' incremental adjustments have no impact on the abnormal net-buying activities of sophisticated investors. These results show that the study's main inference that primarily less sophisticated investors trade on information in pro forma earnings, while more sophisticated investors do not, is robust to concerns about measurement error.

These results have relevance in the post-Enron and post-SOX regulatory and disclosure environment. Legislators and regulators are increasingly concerned regarding the proliferation of pro forma earnings figures published by managers, and early evidence suggests that these disclosures may be misleading and strategically motivated. Thus, our evidence that less sophisticated, primarily individual investors trade on pro forma earnings information, while sophisticated institutional investors do not, is relevant to standard-setters and regulators mandating and monitoring corporate disclosures to protect the interests of potentially less informed investors.

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THE ACCOUNTING REVIEW
Vol. 82, No. 3
2007
pp. 621–650

An Analysis of Forced Auditor Change: The Case of Former Arthur Andersen Clients

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ABSTRACT: This study examines former Arthur Andersen clients and provides evidence on the factors involved in their selection of new auditors after Andersen's collapse. Using a unique dataset that identifies whether former Andersen clients followed their audit team to a new auditor, findings reveal companies with greater agency concerns were more likely to sever ties with their former auditor, whereas those with greater switching costs were more likely to follow their former auditor. We also investigate the effect of the forced auditor change on financial statement quality in an effort to provide insight into the mandatory auditor rotation debate. Using performance-adjusted discretionary accruals as a proxy for reporting quality, our results fail to reveal significant improvements for companies with extreme discretionary accruals that severed ties with Andersen, which is inconsistent with the notion that mandatory rotation improves financial reporting.

Keywords: *auditor selection; mandatory auditor rotation; audit quality; earnings quality; Arthur Andersen.*

Data Availability: *Data are available from public sources.*

I. INTRODUCTION

In this paper, we take advantage of the unique setting created by the collapse of Arthur Andersen (AA) to examine the costs a company faces in selecting a new auditor. While auditing is widely believed to be a means of reducing agency costs, the trade-off among agency and other costs in selecting an auditor is not well understood. In an effort to better

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We thank Paul Allison, Scott Baggett, Dan Dhaliwal (editor), Jagan Krishnan, Karen Nelson, Kevin Raedy, Terry Shevlin (previous editor), Richard Smith, Stefanie Tate, James Weston, Stephen Zeff, two anonymous referees, and workshop participants at Drexel University, University of Massachusetts Lowell, and Southern Methodist University for constructive criticisms and suggestions.

Editor's note: This paper was accepted by Dan Dhaliwal.

Submitted April 2005
Accepted September 2006

understand the complex process of selecting a new auditor, we study company attributes that measure the extent of switching costs (e.g., costs incurred by the client in a new audit engagement, including increased risk of audit failure) and agency costs (forgone agency benefits stemming from greater auditor independence) borne by switching companies.[1]

A change in auditor involves two actions: dismissal/resignation of the current audit firm and the selection of a new auditor. Prior auditor change research has been unable to examine the two actions separately and, therefore, has focused on the joint decision (see Nichols and Smith 1983; Francis and Wilson 1988; Shu 2000; Landsman et al. 2006). AA's collapse forced each of its clients to select a new auditor, creating a setting where a large number of companies switched auditors *for the same reason during the same time period.* Therefore, our sample of former AA clients is homogeneous in the requirement to obtain new auditors, enabling us to create more direct tests of the costs involved in the selection of a new auditor than have been possible in past studies that utilize auditor dismissals and/or resignations.

Although Andersen's demise forced our sample to change auditing firms, companies had the opportunity to follow their former audit team to a new auditor. We capitalize on this setting by noting that companies electing to follow AA were likely trying to minimize the costs associated with changing auditors, whereas companies that severed ties with AA did so presumably because the agency benefits obtained through a new independent auditor outweighed the switching costs. We characterize the follow decision based on the prospective employment of the AA audit team. For example, in Casella Waste Systems' Form 8-K filing on June 13, 2002, the company reports:

> As recommended by the audit committee, the Board of Directors on May 20, 2002, decided to no longer engage its independent accountants, Arthur Andersen LLP, and engaged KPMG LLP ("KPMG") to serve as the Company's independent accountants for the fiscal year ending April 30, 2003 and to audit the Company's financial statements for the fiscal year ended April 30, 2002. The Audit Committee's recommendation to engage KPMG was based on the assumption that certain individuals from Arthur Andersen's Boston, Mass. office, including the team auditing the Company, would join KPMG. That event did not occur. As a result, the Audit Committee subsequently reconsidered its recommendation and, as recommended by the Audit Committee, the Board of Directors on June 13, 2002 decided to no longer engage KPMG, and engaged PricewaterhouseCoopers LLP ("PWC") to serve as the Company's independent accountant for the fiscal year ending April 30, 2003 and to audit the Company's financial statement for the fiscal year ended April 30, 2002.

Ultimately, AA's Boston office became part of PWC rather than KPMG. We argue that companies such as Casella Waste Systems did not switch audit teams, but instead simply transferred their existing audit relationship to a new firm (follow companies). Since other companies clearly severed ties with their former AA audit team (non-follow companies), we have identified an interesting quasi-experimental setting in which to study the cost/benefit relationship underlying the selection of a new auditor.

In our sample of 407 former AA clients, we find that companies with greater switching costs were more likely to follow their former AA audit team to the new auditor. Specifically, companies with more aggressive accruals behavior followed their AA team. This is consistent with a company's attempt to limit the costs of switching by maintaining a relationship with the auditor who originally opined on the company's aggressive behavior. In addition, companies were more likely to follow their AA teams when AA had the largest proportion of clients in the state and industry, which suggests that these companies minimized switching costs. Other measures of switching costs, including the length of time AA had been the auditor and size of the company, are not associated with the decision to follow the AA team.

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[1] Prior research on auditor changes suggests there may be a third cost considered in selection of a new auditor—implicit insurance. Rather than modeling this cost, we hold it constant by only examining switches to the remaining Big 4 auditors, which are likely to provide equivalent implicit insurance.

On the other side of the trade-off, we find that companies with greater agency concerns were more likely to sever ties with AA. Our results are consistent with more complex companies (e.g., companies with less transparent earnings and greater geographic diversity) selecting an auditor that mitigates the greater monitoring costs faced by outside shareholders, which implies minimization of their agency costs. In addition, we find companies with outside blockholders were also more likely to sever ties with AA, consistent with a desire by outside stakeholders to ensure an independent audit. However, we find little evidence that governance mechanisms had an effect on the company's auditor selection. Although the presence of a financial expert on the audit committee had a marginal influence on the committee's choice of an auditor, other board characteristics were unassociated with a company's auditor selection.

Overall, we interpret our evidence as suggesting that switching costs are a major consideration in non-forced auditor change environments, which is consistent with the fact most companies change auditors infrequently. At the same time, we illustrate that in our forced change setting, agency benefits exceed the costs saved by following AA for many sample companies. These results are helpful in understanding the costs and benefits weighed by companies in the selection of an auditor, as well as providing some calibration of the costs and benefits involved in the debate over the mandatory rotation of auditors.

Finally, we supplement the cost trade-off analysis by examining whether AA's collapse led to a change in the financial reporting quality of sample companies. Using our forced change setting, we investigate whether the performance-matched discretionary accrual behavior differed between our follow and non-follow companies. We expect non-follow companies with extreme accruals to exhibit the greatest degree of reversion if the change in auditor is effective in improving financial reporting. However, we find that companies with the lowest relative levels of discretionary accruals, in the final year audited by AA, continued to have relatively low accruals following Andersen's failure, regardless of their follow decision. This suggests the change did not improve the reporting for these companies. In addition, we find that non-follow companies with high discretionary accruals continued to exhibit higher discretionary accruals on average in the first year with their new auditor. In contrast, the follow counterparts exhibited reversion in their aggressive accruals behavior during the year after AA's demise. These findings do not suggest financial reporting quality significantly improved for companies selecting an entirely new auditor, providing evidence that mandatory rotation of auditors may not yield an increase in financial statement quality.

The rest of the paper is organized as follows: in Section II of this paper, we develop our hypotheses and present our research design for testing the cost trade-offs in selecting an auditor. Section III summarizes our sample selection and results. In Section IV, we develop and present our tests of changes in financial reporting. Section V presents our conclusions.

II. AUDITOR SELECTION

Hypotheses Development

Although auditing is widely believed to be a means of reducing agency costs, there is no broad theory on how companies choose a new auditor or weigh the cost/benefit tradeoff in switching auditors. Many papers investigate auditor switches and company characteristics (e.g., Nichols and Smith 1983; Francis and Wilson 1988; Johnson and Lys 1990; Krishnan and Krishnan 1997; Shu 2000; Hackenbrack and Hogan 2002; Sankaraguruswamy and Whisenant 2004). However, they generally have been unable to isolate the effects of the selection of a new auditor from the dismissal/resignation of the current auditor (e.g., opinion shopping and financial reporting disagreements, fees, risk, etc.).[2] As a result, they investigate costs involved in the joint decision of hiring and firing.

In contrast, the unexpected and rapid collapse of Arthur Andersen provides the opportunity to examine a group of companies that switched auditors for the same reason: their former audit firm was forced to stop practicing. We use this forced change to examine a company's selection of a new auditor. Specifically, we investigate which costs factor into a client's decision to either follow its former AA audit team or choose an entirely new audit firm. Prior research on auditor changes and the debate on mandatory auditor rotation suggest three potential costs involved in the selection of a new auditor: switching, agency, and implicit insurance. We hold the latter constant by only examining switches to the remaining Big 4 auditors, allowing us to focus on switching and agency costs.[3]

Ex ante, the relative weighting of switching and agency costs is difficult to predict. The prior literature often focuses on agency costs with virtually no attention given to switching costs since they are extremely difficult to quantify in a non-forced auditor change environment. The fact that auditor changes occur relatively infrequently is consistent with the notion that switching costs are generally high. Said another way, the sporadic nature of auditor switches suggests that the marginal agency benefit gained from changing auditors is significantly less than the cost of switching to that new independent auditor. However, the fact that all companies in our sample were forced to change auditors alters the cost considerations, but at the same time provides us with a rare opportunity to examine whether switching costs truly play a role in the decision to change auditors and, if so, to what extent.

Switching Costs

We define switching costs as the start-up costs incurred by the client for a new audit engagement. These include: (1) costs incurred by the client in educating the auditor about the company's operations, systems, financial reporting practices, and accounting issues, (2) costs incurred by the client in selecting a new auditor (e.g., time spent listening to and reviewing proposals), and (3) an increased risk of audit failure (AICPA 1978; Palmrose 1987; U.S. General Accounting Office [GAO] 2003; Geiger and Raghunandan 2002; Myers et al. 2003).[4]

All else equal, value-maximizing behavior suggests that companies will seek to minimize switching costs. We hypothesize that companies may try to minimize the cost of switching auditors by following their AA audit team because they already possess client and industry-specific knowledge:

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[2] Schwartz and Menon (1985) is a notable exception that examines factors associated with 35 companies that changed auditors because of bankruptcy-related issues.

[3] This assumes that the relative implicit insurance provided by the remaining Big 4 auditors is in fact reasonably equal. This is consistent with prior literature that examines implicit insurance (i.e., Menon and Williams 1994), and which utilizes a Big N/non-Big N designation to test for differences in insurance values.

[4] The U.S. General Accounting Office (GAO 2003) report estimates that mandatory rotation of auditors will increase initial-year audit costs by at least 17 percent of audit fees. This estimate includes increases in support costs (11 percent of initial-year audit fees) and selection costs (6 percent of initial-year audit fees).

> **H1:** The greater the switching costs, the more likely a former AA client will follow its AA audit team to a new auditor, *ceteris paribus*.

The assumption maintained throughout our analysis is that, *ceteris paribus*, following AA has lower switching costs than not following. Educating the audit team about the operations of the business is a time-consuming and costly activity (GAO 2003). Following AA would almost certainly reduce these costs even if the prior audit team was not maintained because, at a minimum, the prior engagement personnel are likely to be available for consultation. Consistent with this notion, the GAO found that Tier 1 public accounting firms "generally saw more potential value in having access to the previous audit team and its audit documentation than in performing additional audit procedures and verification of the public company's data during the initial years of the auditor's tenure" (GAO 2003). Furthermore, anecdotal evidence obtained through discussions with Big 4 audit partners and personnel indicates that former AA audit teams were kept largely intact when a client chose to follow AA.

Agency Costs

Consistent with Jensen and Meckling (1976), we define agency costs as monitoring expenditures by the principal, bonding expenditures by the agent, and loss in welfare experienced by the principal due to the agent not acting in the principal's best interest. Auditing is a means of reducing agency costs through the monitoring of the agent by an independent third-party auditor (Jensen and Meckling 1976; Watts and Zimmerman 1983; among others). Further, the greater the agency costs, the greater the demand for high-quality audits (DeAngelo 1981; Dopuch and Simunic 1982).[5]

The decision to change auditors is frequently cast in terms of mitigating agency costs or improving audit quality (Nichols and Smith 1983; Francis and Wilson 1988; Johnson and Lys 1990; DeFond 1992). In our setting, agency conflicts at the individual company level did not change. Instead, the empirical evidence documenting negative market reactions for AA clients upon the collapse of AA (Chaney and Philipich 2002; Krishnamurthy et al. 2006; Asthana et al. 2004) indicates that the perceived quality of the AA audit had suddenly declined. As such, Andersen clients lost some agency benefit inherent in their relationship with their auditor. Further, duration analyses examining cross-sectional differences in the length of time former AA clients took to select a new auditor support the notion that clients were concerned about the perceived quality of AA's audits, and illustrate that companies with greater agency conflicts dismissed AA sooner (Chang et al. 2003; Barton 2005). Given these findings we hypothesize:

> **H2:** The greater the agency conflicts, the more likely a former AA client will not follow its AA audit team to a new auditor, *ceteris paribus*.

Research Design

We model the decision to follow AA personnel as a function of variables that capture the degree of a company's switching and agency costs. To examine this decision, we utilize factors suggested in prior literature on auditor changes, mandatory auditor rotation, and corporate governance:

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[5] Consistent with DeAngelo (1981) and DeFond (1992), we define audit quality as the probability that an audit firm will detect and report "material breaches in the accounting system."

$$\begin{aligned} FOLLOW = {} & \textstyle\sum_I \alpha_I + \gamma_1 FEE\_EXPERT + \gamma_2 CLIENTS + \gamma_3 TENURE + \gamma_4 SIZE \\ & + \gamma_5 TRANSPARENCY + \gamma_6 COMPLEX + \gamma_7 ACCRUAL \\ & + \gamma_8 INSIDER + \gamma_9 LEVERAGE + \gamma_{10} BLOCK + \gamma_{11} INDAUDIT \\ & + \gamma_{12} ACCT\_FE + \gamma_{13} ROA + \gamma_{14} LOSS + \varepsilon \end{aligned} \tag{1}$$

where all variables are measured as of the final year audited by AA and are defined as follows (Compustat data items in parentheses):

FOLLOW = 1 if the client followed AA, 0 otherwise;
FEE_EXPERT = 1 if AA had the greatest total audit fees in an industry and state, 0 otherwise;
CLIENTS = 1 if AA had the most clients in an industry and state, 0 otherwise;
TENURE = number of years audited by AA per Compustat;
SIZE = natural logarithm of total assets (#6);
TRANSPARENCY = descending decile rank of absolute value of residual from regression of annual returns on annual earnings (#18), and changes in annual earnings, both scaled by total assets (#6) and *SIZE*;
COMPLEX = $\sum_{i=1}^{N}\left[\left(LN\left(\frac{TotalSales}{Segment_i}\right)\right)\frac{Segment_i}{TotalSales}\right]$ where *TotalSales* is company sales revenue for 2001 and $Segment_i$ represents the sales for a specific geographic segment of the business per Compustat;
ACCRUAL = performance-adjusted discretionary accruals;
INSIDER = 1 if an insider per Spectrum holds at least 5 percent of the outstanding shares, 0 otherwise;
LEVERAGE = ratio of debt (#9 + #34) to total assets (#6);
BLOCK = 1 if an outside blockholder per Spectrum holds at least 5 percent of the outstanding shares, 0 otherwise;
INDAUDIT = 1 if audit committee at the time the decision was made to dismiss AA had 100 percent outside members, 0 otherwise;
ACCT_FE = 1 if an accounting financial expert was on the audit committee, 0 otherwise;
ROA = return on assets, defined as net income before extraordinary items (#18) divided by ending total assets (#6);
LOSS = 1 if *ROA* < 0, 0 otherwise; and
I = denotes industry as defined in Barth et al. (1998).[6]

We classify a former AA client as following the AA audit team (*FOLLOW* = 1) if the new auditor acquired the AA audit practice corresponding to the office (city) indicated on the client's audit report. For example, KPMG acquired AA's Philadelphia office. If an AA client whose audit opinion was signed "Philadelphia" chose KPMG as its new auditor, then we assume it followed its AA audit team. If a client chose Ernst & Young, we assume that it did not follow its AA audit team (*FOLLOW* = 0). We were unable to categorize some large AA offices such as New York, Houston, and Chicago (AA's headquarters) and, therefore, have excluded these offices' clients from our analysis.[7] Although these exclusions mean that we may not be able to generalize our findings to all of AA's former clients, we are unaware of any systematic biases within our sample that influence our results.

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[6] Throughout the paper we utilize the Barth et al. (1998) industry classifications for all calculations.

Switching Costs

Our first measure of switching costs involves industry expertise, where hiring the industry expert reduces start-up costs for clients. If AA was the industry expert, then we expect switching costs to be reduced by following the AA team to the new audit firm, leading to a positive relation between expertise and following AA. Since auditor industry expertise is unobservable, we utilize two proxies found in prior research (see for example, Palmrose 1986; Hogan and Jeter 1999; Balsam et al. 2003; Francis, Reichelt, and Wang 2005) that measure industry expertise as a function of experience auditing a larger *number* of clients and/or from auditing large clients.

Similar to Francis, Reichelt, and Wang (2005), our first measure, *FEE_EXPERT*, equals 1 if AA had the greatest audit fees in an industry and state, and 0 otherwise. Industries are defined as in Barth et al. (1998), and the state is obtained from the final audit opinion signed by AA. Our second measure, *CLIENTS*, is based on the number of clients rather than audit fees. *CLIENTS* equals 1 if AA had the most clients in an industry and state, and 0 otherwise.[8] We use the Audit Analytics database, which tracks the office signing the audit report along with audit fee-related information, to construct our measures. We anticipate a positive relation between following AA and measures of Andersen's expertise.

TENURE is the number of years AA performed the audit per Compustat. DeAngelo (1981) suggests there may be a relationship-specific investment between auditor and client where, in order to recover start-up costs, the two firms are better off maintaining their relationship, at least in the early years. In addition, Williams (1988) finds that longevity on an engagement is significantly positive in a stepwise logistic analysis of factors associated with a change in auditor. Together these results suggest that companies with shorter *TENURE* will be more likely to follow AA. On the other hand, companies with extended *TENURE* may find it costly to switch since they have developed relations with their auditor over a long period of time (the audit firm has moved to the top of the learning curve). Since the direction of its association with *FOLLOW* is ambiguous, we do not make a sign prediction for this variable.

We predict a positive coefficient on *SIZE*, defined as the natural logarithm of total assets, because switching costs are expected to be higher for larger clients (DeAngelo 1981).[9] Further, *SIZE* may act as a proxy for client complexity and geographic constraints that we expect to be positively correlated with start-up costs associated with switching auditors. *SIZE*, as described below, is also related to agency costs.

All else equal, we anticipate that the more complex a company, the greater the cost of switching auditors. We use two measures to capture the complexity of a company's audit.

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[7] These offices often did not transfer all personnel to a single new audit firm, which made the follow/non-follow designation difficult to make. Further, our attempts to contact firm representatives related to the unclassified offices were not successful.

[8] *CLIENTS* is similar to measures of expertise utilized in Balsam et al. (2003). However, Balsam et al. (2003) defined expertise on a national rather than state basis.

[9] An alternative interpretation of a positive association would be that *SIZE* is a proxy for audit fee potential consistent with Simunic (1980) and, therefore, simply represents the effort of former AA partners to maintain their most lucrative clients.

First, financial reporting transparency is measured as the degree to which a company's accounting summary measures correlate with its economic value. The variable *TRANSPARENCY* is defined as the decile rank (in descending order) of the absolute value of the residual from the following cross-sectional regression estimated for fiscal year 2001:

$$RETURN = \sum_{I}\alpha_{I} + \gamma_1 ROA + \gamma_2 CHGNI + \gamma_3 SIZE + \varepsilon \tag{2}$$

where:

RETURN = buy and hold return over the fiscal year utilizing CRSP monthly returns;
ROA = return on assets, defined as net income before extraordinary items (#18) divided by ending total assets (#6);
CHGNI = net income (#18) in current year less net income in prior year divided by ending total assets (#6);
SIZE = natural logarithm of total assets (#6); and
I = denotes industry as defined in Barth et al. (1998).

Observations in the highest decile are those with the highest transparency, while those in the lowest decile are those with the lowest transparency. Consistent with our use of the variable as a measure of company transparency, similar measures are utilized in other studies (Easton and Harris 1991; Bushman et al. 2004; Barth et al. 2005; Lang and Lundholm 1996; Healy et al. 1999) to illustrate that companies with greater transparency have lower costs of capital, greater analyst following, and greater disclosure of management forecasts. We predict a negative coefficient for *TRANSPARENCY* because companies with lower transparency are more difficult to audit and, therefore, should find it less costly to follow their AA team.[10] As described below, *TRANSPARENCY* is also related to agency costs.

Our second proxy for the extent of the company's audit complexity, *COMPLEX*, is measured as:

$$\sum_{i=1}^{N}\left[\left(LN\left(\frac{TotalSales}{Segment_i}\right)\right)\frac{Segment_i}{TotalSales}\right] \tag{3}$$

where *TotalSales* is company sales revenue for 2001 (representing the last year audited by AA) and *Segment*$_i$ represents the sales for a specific geographic segment of the business per Compustat (Bushman et al. 2002). Chung and Kallapur (2003), Barton (2001), and Palepu (1985) use similar measures to capture segment diversification. *COMPLEX* accounts for the number of geographic segments and the degree of diversity in sales across these segments. While a greater number of geographic segments leads to higher values of *COMPLEX*, companies with relatively equal sales levels across their segments obtain the highest values. This captures the notions that (1) a company with several geographic segments is more difficult to audit than a company with one segment, and (2) a company with relatively equal sales across its geographic segments is more difficult to audit than a company with a similar number of geographic segments, but whose sales occur predominantly in one location. We predict companies with higher values of *COMPLEX* will be more likely to follow AA, since these companies are more challenging to audit and, therefore, have higher switching costs. *COMPLEX* is also related to agency costs, which we describe below.

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[10] Inferences are unaltered if we utilize the actual residual value rather than the decile rank.

Our final measure of switching costs is *ACCRUAL*, which is defined as performance-adjusted discretionary accruals. Specifically, we first estimate cross-sectional modified Jones (1991) model regressions on an industry basis, where industry designation follows Barth et al. (1998), for fiscal year 2001 for all companies on Compustat with the necessary data.[11] Companies are then ranked within industries into deciles based on *ROA*. Sample companies' discretionary accruals are adjusted by the median industry-*ROA* decile discretionary accrual (see Francis, LaFond, Olsen, and Schipper 2005).[12] Bradshaw et al. (2001) finds that auditor changes are *less* likely for high accrual companies, suggesting that it is more costly for these companies to voluntarily change auditors. In the current context, we expect companies with higher values of *ACCRUAL* (most aggressive relative to performance-matched companies) to attempt to reduce the costs of switching auditors by following AA, resulting in a positive prediction for the *ACCRUAL* coefficient. Alternatively, DeFond and Subramanyam (1998) finds companies changing auditors have negative discretionary accruals on average and attribute the change to overly conservative accounting required by the incumbent auditor. We expect companies with lower values of *ACCRUAL* (most conservative relative to performance-matched companies) to find it less costly to change auditors, thereby leading to the same positive coefficient prediction.

Agency Costs

SIZE is frequently used as a proxy for agency concerns. Barton (2005) uses company size as a proxy for reputation costs from the AA collapse. He finds that larger AA clients switched to a new auditor earlier than smaller companies and argues that this result is attributable to the fact that larger companies are subject to greater reputation costs. In addition, *SIZE* may also measure the diffusion of ownership and related agency costs. In contrast to our switching cost predictions, if agency costs dominate the decision to switch auditors, we expect *SIZE* to be negatively related to the likelihood of following the AA team.

The inability to perfectly observe the actions of managers by outside parties increases agency costs (Jensen and Meckling 1976). *TRANSPARENCY* and *COMPLEX* capture company financial reporting and audit complexity. As such, they measure the degree of difficulty outside parties have in monitoring management. Companies with lower (higher) values of *TRANSPARENCY* (*COMPLEX*) are less transparent (more complex) and more difficult to monitor, which leads to a greater demand for a high-quality audit and, as such, a greater likelihood of severing ties with AA. We expect *TRANSPARENCY* (*COMPLEX*) to be positively (negatively) associated with the decision to follow AA under the agency hypothesis, which is contrary to our switching cost expectations.

Jensen and Meckling (1976) shows that higher management ownership leads to greater alignment of interests with outside owners and, hence, lower agency conflicts. Using the Thomson Spectrum database, we define *INSIDER* as a dichotomous variable equaling 1 if an insider holds at least 5 percent of the outstanding shares, and 0 otherwise. Findings in prior research on the relation between insider ownership and auditor changes have been mixed. Francis and Wilson (1988) find no significant relation between insider ownership and the quality of the successor auditor, while Simunic and Stein (1987) find a negative association and Eichenseher and Shields (1989) find a positive association.[13] If low insider ownership is indicative of greater agency problems, then we predict a negative relation between *INSIDER* and following AA.

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[11] We estimate discretionary accruals as the residual from the regression of total accruals on a constant term, property, plant, and equipment, and the difference between the change in sales and accounts receivable all scaled by total assets.

[12] Performance matching mitigates concerns about bias in the Jones model estimates related to performance documented by Dechow et al. (1995), along with controlling for any potential systematic differences in estimates of discretionary accruals across industries. See Kothari et al. (2005) for further discussion.

LEVERAGE (debt-to-asset ratio) captures both the degree of agency conflicts between stock and debt holders and the agency costs involved in monitoring by debt holders. DeFond (1992) argues that companies with greater leverage tend to switch to higher-quality audit firms because of the monitoring performed by bondholders. If debt holders view the demise of AA as indicative of low audit quality, then we predict the greater the *LEVERAGE* the less likely companies will be to follow AA.

Costs to monitor and influence management actions are increasing with the diffusion of equity ownership. As such, blockholders' ownership leads to economies of scale in terms of managerial monitoring. However, concentrated share ownership is only needed if there is some reason to believe that managerial monitoring has been inadequate (e.g., a weak board). As such, blockholder ownership is suggestive of the presence of agency issues. Consistent with prior research on auditor changes, we include *BLOCK*, which equals 1 if an outside blockholder per Spectrum holds at least 5 percent of the outstanding shares, and 0 otherwise.[14] An explanation consistent with this agency cost argument is that blockholders may be more likely to force companies to sever ties with AA to ensure the quality/independence of their successor auditor. If blockholder ownership is indicative of greater agency costs, then we expect companies with blockholders to be less likely to follow AA.

Another form of monitoring relates to the independence and financial reporting expertise of companies' audit committees. In *Standards Relating to Listed Company Audit Committees*, the SEC suggests that the audit committee serves a central role in independent review and oversight of a company's independent auditors. Given this, we include two measures of audit committee monitoring as utilized in DeFond et al. (2005). First, *INDAUD* measures the independence of the audit committee and is equal to 1 if all members are independent. Our second measure related to the audit committee, *ACCT_FE*, is a proxy for financial expertise. Consistent with DeFond et al. (2005), we define *ACCT_FE* as equal to 1 if anyone on the audit committee has experience as a public accountant, auditor, principal or chief financial officer, controller, or chief accounting officer. DeFond et al. (2005) illustrates that only companies electing accounting financial experts (as opposed to the more inclusive definition eventually adopted in Sarbanes-Oxley that includes individuals responsible for managing financial experts, among other less stringent criteria) to their audit committees will experience significantly positive cumulative abnormal returns around the announcement of said election.

Although corporate governance is most often utilized in discussions concerning agency conflicts, *a priori*, it is difficult to make a signed prediction on the governance-related variables in our setting. For instance, companies with more independent audit committee members and/or those with financial experts might want to ensure the independence of their auditor and, therefore, select an auditor unaffiliated with AA. Alternatively, these governance indicators might be consistent with audit committee members who have monitored the audit relationship effectively and who, therefore, may be more likely to follow AA in order to minimize the costs associated with obtaining a new auditor. Given these counter arguments, we make no sign predictions for *INDAUD* or *ACCT_FE*.

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[13] In related research, Barton (2005) finds that companies with smaller managerial ownership were more likely to dismiss AA sooner.

[14] Francis and Wilson (1988) and Palmrose (1984) use similar measures, but neither finds a significant relation between diffusion of ownership and choice of auditor.

Control Variables

We include industry-fixed effects, where industry is defined as in Barth et al. (1998) to allow for systematic differences in industries' switching behaviors that are unrelated to our agency and switching cost arguments. We also utilize *ROA* and *LOSS* as control variables. Landsman et al. (2006) and Schwartz and Menon (1985) find that companies with poor financial performance are more likely to change auditors. In our context, this suggests that poorly performing companies may be less likely to follow AA, but classifying this prediction as related to agency or switching costs is difficult. We therefore include *ROA* and *LOSS* as measures of financial performance, but make no predictions as to the sign of the coefficients. Figure 1 summarizes our sign predictions under the two hypotheses for all of the variables.

III. SAMPLE SELECTION AND RESULTS

Sample Selection

In constructing our sample, we used Compustat to identify U.S. companies that were audited in fiscal year 2001 by AA. Next, we reviewed each company's audit report to determine which office (city) had performed the audit. Then we hand-collected information concerning the acquisition of AA offices by other auditors from a variety of sources including audit firm press releases, AA client Form 8-Ks relating to the choice of a new auditor, and representatives from two of the remaining Big 4 audit firms. Through this process we were able to classify 561 former AA clients as either following AA personnel to a new auditor or completely severing ties with their AA audit team. We eliminated 29 observations where the corresponding AA practice was acquired by a non-Big 4 auditor.[15] Another 127 observations with missing data were eliminated leaving us with 407 former AA clients that selected one of the remaining Big 4 auditors. A total of 226 companies are classified as following their AA audit teams and 181 classified as choosing not to follow. Table 1 provides a summary of the sample selection process.

Panel B of Table 1 provides a timeline along with a cumulative frequency count of when companies in our sample switched auditors. Auditor changes in our sample range from February 12, 2002 to August 2, 2002. Most companies in our sample (69 percent) switched between the indictment on March 14, 2002, and the conviction on June 15, 2002, with only 2 percent switching prior to the indictment date and 29 percent switching after the conviction date.

The industry composition for the sample is illustrated in Table 1, Panel C, which also reports the percentage of companies in a given industry on Compustat that were audited by a Big 5 auditor during fiscal year 2001. The panel illustrates that the follow and non-follow samples have very similar industry compositions when compared to each other and to the Compustat sample. Although this implies that any results are not likely to be biased because of systematic movements by any particular industry, we control for industry-fixed effects in our tests.

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[15] We have relatively little information concerning AA personnel switches to non-Big 4 auditors, which reduces our ability to generalize to this population. Furthermore, the extant literature suggests that switches to non-Big 4 auditors occur for significantly different reasons than upward or lateral movements (Johnson and Lys 1990). Although Landsman et al. (2006) illustrate downward and lateral changes involving Big N auditors are influenced by similar characteristics, we focus on the Big 4 sample in order to avoid concerns about downward switches biasing our results. Nevertheless, results are unchanged when companies selecting non-Big 4 auditors are included.

FIGURE 1
Hypotheses and Sign Predictions

Variable[a]	Switching Costs	Agency Costs
FEE_EXPERT	+	
CLIENTS	+	
TENURE	?	
SIZE	+	–
TRANSPARENCY	–	+
COMPLEX	+	–
ACCRUAL	+	
INSIDER		+
LEVERAGE		–
BLOCK		–
INDAUDIT	?	?
ACCT_FE	?	?
ROA	?	?
LOSS	?	?

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[a] Variable Definitions:

FOLLOW = 1 if a client is designated as following their former AA audit team to a new auditor, 0 otherwise;
FEE_EXPERT = 1 if AA had the greatest total audit fees in an industry and state, 0 otherwise;
CLIENTS = 1 if AA had the greatest number of clients in an industry and state, 0 otherwise;
TENURE = number of years audited by AA per Compustat;
SIZE = natural logarithm of total assets (data6);
TRANSPARENCY = descending rank of the absolute value of the residual from a cross-sectional regression of annual returns on *ROA*, changes in earnings, *SIZE*, and industry-fixed effects;
COMPLEX = geographic sales diversity of a company;
ACCRUAL = performance-matched discretionary accruals utilizing the modified Jones (1991) model and adjusting by the median discretionary accruals for companies in the same industry and *ROA* decile;
INSIDER = 1 if an insider has 5 percent or more of the stock per Spectrum, 0 otherwise;
LEVERAGE = total debt divided by total assets;
BLOCK = 1 if an outside blockholder has 5 percent or more of the stock per Spectrum, 0 otherwise;
INDAUDIT = 1 if the audit committee responsible for making the follow decision was 100 percent independent, 0 otherwise;
ACCT_FE = 1 if the audit committee has an accounting financial expert, 0 otherwise;
ROA = net income before extraordinary items divided by ending total assets; and
LOSS = 1 if *ROA* is less than 0, 0 otherwise.

Results

Univariate

Table 2 provides descriptive statistics for both the companies that followed and those that did not follow their AA audit teams. AA was more likely to be the industry leader in terms of number of clients in a given state for the follow companies (28 percent) than for non-follow companies (14 percent). Companies that chose to follow AA were more transparent with a mean of 5.75 compared to companies that did not follow AA with a mean of 5.10 (p-value 0.02). In addition, companies that followed AA were less complex than

TABLE 1
Sample Selection and Industry Composition

Panel A: Sample Selection

Compustat AA companies		1,086
Less		
Foreign companies	24	
Filing information unavailable	3	
Not audited by AA prior to 10/15/01	16	
	43	
Switch Sample		1,043
Less		
Insufficient information to classify follow or not	480	
Non-Big 4 observations	29	
Missing regression information	127	
	636	
Total Sample		407

Panel B: Timeline of Key Switching Dates and Decision to Change Auditor

Event	**Timeline**	**Cumulative # of Non-Follow Companies that Have Changed**	**Cumulative # of Follow Companies that Have Changed**
Enron announced restatement	10/16/01	0	0
AA disclosed shredding	01/10/02	0	0
AA indicted	03/14/02	1	4
AA convicted	06/15/02	134	153
AA ceased practicing	08/31/02	181	226

Panel C: Industry Composition

	Non-Follow		**Follow**		**Compustat**
Industry	**Number**	**Freq. (%)**	**Number**	**Freq. (%)**	**Freq. (%)**
Chemicals	3	1.7	5	2.2	2.1
Computers	40	22.1	45	19.9	16.5
Durables	42	23.2	47	20.8	19.4
Extraction	7	3.9	9	4.0	3.5
Finance	6	3.3	2	0.9	6.1
Food	1	0.5	3	1.3	2.0
Insurance	4	2.2	4	1.8	5.0
Mining	4	2.2	4	1.8	2.4
Other	0	0.0	1	0.4	1.0
Pharmaceuticals	6	3.3	18	8.0	5.9
Retail	19	10.5	19	8.4	9.4
Service	19	10.5	29	12.8	9.5
Textiles	5	2.8	10	4.4	4.2
Transportation	11	6.1	21	9.3	7.9
Utilities	14	7.7	9	4.0	5.1
Total	181	100.0	226	100.0	100.0

(continued on next page)

TABLE 1 (continued)

This table provides descriptive statistics concerning the sample selection and industry composition of the sample. Industry membership is determined by primary SIC code as follows: Agriculture (0100–0999), Mining and construction (1000–1999, excluding 1300–1399), Food (2000–2111), Textiles and printing/publishing (2200–2780), Chemicals (2800–2824, 2840–2899), Pharmaceuticals (2830–2836), Extractive (2900–2999, 1300–1399), Durable manufacturers (3000–3999, excluding 3570–3579 and 3670–3679), Computers (7370–7379, 3570–3579, 3670–3679), Transportation (4000–4899), Utilities (4900–4999), Retail (5000–5999), Finance (6000-6411), Insurance (6500-6999), Services (7000–8999, excluding 7370–7379), and Other (> 9000). Data for the "Compustat" column are obtained from Compustat, and are based on all companies for fiscal year 2001 with a Big N auditor.

companies that did not follow AA, with mean values of 0.27 and 0.36, respectively (p-value 0.05). Further, companies following AA had higher performance-adjusted discretionary accruals with a mean of 0.01 than their non-follow counterparts with a mean of −0.04 (p-value 0.00). As stipulated by the listing requirements on the stock exchanges at the time, both samples exhibit relatively high proportions of entirely independent audit committees (87 percent for non-follow and 80 percent of follow companies) with the non-follow companies being marginally more likely to have an entirely independent audit committee (p-value 0.06).

Neither the follow nor non-follow companies appears to have performed very well in the final year audited by AA as indicated by mean *ROA*s (−0.17 and −0.10 for non-follow and follow companies, respectively) and the proportion of loss companies (49 and 46 percent for non-follow and follow companies, respectively). However, the median *ROA*s are small and positive, suggesting a need to control for extreme negative performance.

In unreported analyses, we find significant correlations between *FOLLOW* and *CLIENTS*, *TRANSPARENCY*, *COMPLEX*, *ACCRUAL*, and *INDAUDIT*. All are in the same direction as the univariate tests in Table 2 with *ACCRUAL* exhibiting the largest correlation (0.14 Pearson) in absolute magnitude with *FOLLOW*. Tests of multicollinearity for all variables in Table 2 reveal the highest variance inflation factor is 2.1 for *CLIENTS*, which is well below 10.0, the level designated in Belsley et al. (1980) as cause for concern.

Multivariate

Table 3 presents logistic regression results for our follow/non-follow model. Coefficients on *CLIENTS*, *ACCRUAL*, and *ACCT_FE* are consistent with the switching costs argument presented in H1. The positive coefficient on *CLIENTS* indicates that companies were more likely to follow AA in a state/industry where AA had the greatest number of clients, consistent with clients minimizing switching costs by following the expert. *CLIENTS* also may be capturing a "lack of competition," whereby companies may not have had many alternatives other than to follow AA in areas/industries where AA audited the most clients. This latter interpretation appears appropriate given that the results in Table 3 indicate that the odds of following AA by companies in states/industries where AA had the most clients increase by 264 percent.[16] Under both interpretations, *CLIENTS* captures increased switching costs which, in turn, provide impetus for following the AA team.

The significantly positive coefficient on *ACCRUAL* illustrates that companies with higher performance-matched discretionary accruals were more likely to follow AA, which is consistent with the switching costs hypothesis. The findings indicate a one standard

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[16] The unconditional odds of following AA is 1.19-to-1, which is obtained by dividing the frequency of following documented in Table 1 (226) by the frequency of not following (181).

TABLE 2
Descriptive Statistics of Regression Variables

	Non-Follow Sample (n = 181)			Follow Sample (n = 226)			Test of Differences[b]	
Variable[a]	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**
FEE_EXPERT	0.29	0.00	0.43	0.33	0.00	0.45	0.41	0.41
CLIENTS	0.14	0.00	0.33	0.28	0.00	0.41	0.01	0.01
TENURE	10.77	8.00	7.86	10.70	8.00	7.64	0.93	0.63
SIZE	5.63	5.56	1.70	5.66	5.40	1.87	0.87	0.59
TRANSPARENCY	5.10	5.00	2.75	5.75	6.00	2.89	0.02	0.04
COMPLEX	0.36	0.00	0.46	0.27	0.00	0.41	0.05	0.05
ACCRUAL	−0.04	−0.03	0.14	0.01	0.00	0.13	0.00	0.06
INSIDER	0.19	0.00	0.40	0.23	0.00	0.42	0.43	0.43
LEVERAGE	0.17	0.07	0.21	0.20	0.14	0.22	0.12	0.21
BLOCK	0.20	0.00	0.40	0.16	0.00	0.37	0.36	0.36
INDAUDIT	0.87	1.00	0.34	0.80	1.00	0.40	0.06	0.06
ACCT_FE	0.36	0.00	0.48	0.41	0.00	0.49	0.28	0.28
ROA	−0.17	0.01	0.66	−0.10	0.01	0.37	0.16	0.29
LOSS	0.49	0.00	0.50	0.46	0.00	0.50	0.53	0.53

[a] Variable Definitions:

FOLLOW = 1 if a client is designated as following their former AA audit team to a new auditor, 0 otherwise;
FEE_EXPERT = 1 if AA had the greatest total audit fees in an industry and state, 0 otherwise;
CLIENTS = 1 if AA had the greatest number of clients in an industry and state, 0 otherwise;
TENURE = the number of years audited by AA per Compustat;
SIZE = natural logarithm of total assets (data6);
TRANSPARENCY = descending rank of the absolute value of the residual from a cross-sectional regression of annual returns on *ROA*, changes in earnings, *SIZE*, and industry-fixed effects;
COMPLEX = geographic sales diversity of a company;
ACCRUAL = performance-matched discretionary accruals utilizing the modified Jones (1991) model and adjusting by the median discretionary accruals for companies in the same industry and *ROA* decile;
INSIDER = 1 if an insider has 5 percent or more of the stock per Spectrum, 0 otherwise;
LEVERAGE = total debt divided by total assets;
BLOCK = 1 if an outside blockholder has 5 percent or more of the stock per Spectrum, 0 otherwise;
INDAUDIT = 1 if the audit committee responsible for making the follow decision was 100 percent independent, 0 otherwise;
ACCT_FE = 1 if the audit committee has an accounting financial expert, 0 otherwise;
ROA = net income before extraordinary items divided by ending total assets; and
LOSS = 1 if *ROA* is less than 0, 0 otherwise.

[b] Test of Differences presents the associated p-values from the comparison of Non-Follow and Follow companies' mean (t-test) and median (Wilcoxon test) values.

TABLE 3
Logistic Regression of Follow on Measures of Switching and Agency Costs

$$\begin{aligned} FOLLOW = {} & \textstyle\sum_{I}\alpha_{I} + \gamma_{1}FEE\_EXPERT + \gamma_{2}CLIENTS + \gamma_{3}TENURE + \gamma_{4}SIZE \\ & + \gamma_{5}TRANSPARENCY + \gamma_{6}COMPLEX + \gamma_{7}ACCRUAL + \gamma_{8}INSIDER \\ & + \gamma_{9}LEVERAGE + \gamma_{10}BLOCK + \gamma_{11}INDAUDIT + \gamma_{12}ACCT\_FE \\ & + \gamma_{13}ROA + \gamma_{14}LOSS + \varepsilon \end{aligned}$$

Variable[a]	Sign Predictions: Switching	Sign Predictions: Agency	Coeff. Est.	p-value	ΔOdds[b]
FEE_EXPERT	+		−0.04	0.90	−0.04
CLIENTS	+		1.29	0.00	2.64
TENURE	?		−0.01	0.35	−0.11
SIZE	+	−	0.01	0.96	0.01
TRANSPARENCY	−	+	0.12	0.00	0.39
COMPLEX	+	−	−0.60	0.04	−0.23
ACCRUAL	+		3.29	0.00	0.55
INSIDER		+	0.20	0.47	0.22
LEVERAGE		−	0.52	0.42	0.12
BLOCK		−	−0.52	0.05	−0.41
INDAUDIT	?	?	−0.48	0.12	−0.38
ACCT_FE	?	?	0.41	0.08	0.50
ROA	?	?	0.28	0.34	0.15
LOSS	?	?	−0.29	0.28	−0.25

n Follow	226
n Non-Follow	181
Pseudo R^2	0.20
Hosmer-Lemeshow p-value[c]	0.47
ROC curve statistic[d]	0.74

This table presents binary logistic results modeling the probability that a client followed their former AA audit team to a new auditor (*FOLLOW*) versus the reference category of deciding to sever ties with AA (*NON-FOLLOW*).

Reported p-values are based on two-tailed tests.

The model includes unreported industry-fixed effects.

[a] Variable Definitions:

FOLLOW = 1 if a client is designated as following their former AA audit team to a new auditor, 0 otherwise;
FEE_EXPERT = 1 if AA had the greatest total audit fees in an industry and state, 0 otherwise;
CLIENTS = 1 if AA had the greatest number of clients in an industry and state, 0 otherwise;
TENURE = the number of years audited by AA per Compustat;
SIZE = natural logarithm of total assets (data6);
TRANSPARENCY = descending rank of the absolute value of the residual from a cross-sectional regression of annual returns on *ROA*, changes in earnings, *SIZE*, and industry-fixed effects;
COMPLEX = geographic sales diversity of a company;
ACCRUAL = performance-matched discretionary accruals utilizing the modified Jones (1991) model and adjusting by the median discretionary accruals for companies in the same industry and *ROA* decile;
INSIDER = 1 if an insider has 5 percent or more of the stock per Spectrum, 0 otherwise;
LEVERAGE = total debt divided by total assets;
BLOCK = 1 if an outside blockholder has 5 percent or more of the stock per Spectrum, 0 otherwise;

(*continued on next page*)

TABLE 3 (continued)

INDAUDIT = 1 if the audit committee responsible for making the follow decision was 100 percent independent, 0 otherwise;
ACCT_FE = 1 if the audit committee has an accounting financial expert, 0 otherwise;
ROA = net income before extraordinary items divided by ending total assets; and
LOSS = 1 if *ROA* is less than 0, 0 otherwise.

[b] ΔOdds represents the change in odds of following AA given a standard deviation change in the independent variable of interest for continuous variables and relative to the 0 category for all indicator variables. The unconditional odds of following AA is 1.19-to-1.

[c] The Hosmer-Lemeshow test is a measure of the goodness of fit of the model that is developed by comparing the expected versus observed frequencies across intervals that are determined using the probability estimates obtained from the model. The null hypothesis is that the model has an appropriate fit.

[d] The ROC curve statistic measures the area under the Receiver Operating Characteristics curve, which provides an assessment of the model's ability to discriminate between those subjects that meet the condition of interest versus those that do not. Hosmer and Lemeshow (2000) indicate a statistic of 0.70 or greater indicates acceptable model discrimination.

deviation increase in *ACCRUAL* results in a 55 percent increase in the odds of following AA. This implies that companies that were more aggressive with their financial reporting, relative to their performance- and industry-matched peers, wanted to maintain their relationship with the auditor that originally opined on their reports. Alternatively, those companies whose discretionary accruals were lower than their performance-matched counterparts were more likely to sever ties with AA. In Section IV, we address whether these accrual patterns persist after the forced auditor change.

The presence of an accounting financial expert on the audit committee (*ACCT_FE*) is also marginally associated with a company's proclivity to follow AA (p-value of 0.08). All else equal, companies with an accounting financial expert had increased odds of following AA by 50 percent. This suggests that accounting financial experts did not view quality problems at Andersen to be endemic and, therefore, recognized that companies could minimize switching costs by maintaining relations with their current audit personnel.

In contrast, the signs of the coefficients on *TRANSPARENCY*, *COMPLEX*, and *BLOCK* are consistent with the agency costs hypothesis. The positive (negative) coefficient on *TRANSPARENCY* (*COMPLEX*) is significant, which indicates that less transparent (more complex) companies were more likely to not follow their AA audit team because public perception of the lack of Andersen audit quality was simply too costly, implying that the agency costs outweighed the switching costs. A one standard deviation increase in *TRANSPARENCY* (*COMPLEX*) results in a 39 percent increase (23 percent decrease) in the odds of following AA. These results reinforce the arguments made by Chaney and Philipich (2002) and Krishnamurthy et al. (2006) that investors perceived audit quality issues to be systemic at AA.

Finally, the coefficient on *BLOCK* is negative and significant, suggesting that companies with greater agency issues, as evidenced by the presence of outside blockholders, were more likely to switch away from AA. The ΔOdds indicates that companies with blockholders were 41 percent less likely to follow AA than those without blockholders. This supports the agency costs hypothesis, whereby monitoring by outside blockholders led companies to select more independent successor auditors.

The remaining variables are not significantly different from zero. For variables with indeterminate sign predictions (i.e., *TENURE*, *ROA*, *LOSS*), the lack of significance indicates that auditor tenure and company performance were equally distributed across the follow and non-follow samples. For those variables with sign predictions for both hypotheses (i.e., *SIZE*) insignificance suggests the relative weighting of agency and switching costs were equal.

Overall, our model appears to appropriately capture variation in the dependent variable as evidenced by the inability to reject the null of an appropriate model fit indicated by the Hosmer and Lemeshow test (p-value 0.47). Similarly, the ROC curve analysis, with a statistic of 0.74, provides evidence that our model exhibits adequate ability to discriminate between the different companies (Hosmer and Lemeshow [2000] suggest a statistic of 0.70 or better indicates acceptable performance).

In an effort to understand whether switching cost motivations exceeded agency cost considerations or *vice versa*, in unreported analyses we standardized all variables reported in Table 3 and estimated whether the summation of the switching cost variables (*CLIENTS*, *ACCRUAL*, and *ACCT_FE*) was significantly different than the sum of coefficients that are consistent with agency costs (*TRANSPARENCY*, *COMPLEX*, and *INSIDER*), appropriately accounting for the signs of the coefficients.[17] The results fail to reject the null that switching and agency costs are equal (p-value 0.27).[18] We interpret this as evidence of switching costs constituting a major consideration in non-forced change environments, which is consistent with the observation that auditor changes are an infrequent occurrence for most companies. At the same time, when forced to change auditors, many companies viewed the agency benefits as outweighing the savings from following AA.

Multinomial Logistic Regression

The above logistic analysis allows us to study only the variation in the dichotomous follow or not-follow decision. However, Barton (2005) and Chang et al. (2003) find that there were systematic differences in former AA clients that varied directly with the length of time between the Enron restatement announcement and the date companies selected a new auditor. The results from these two papers are generally consistent with companies facing greater agency costs switching auditors earlier. If true, then this suggests that our findings could be a reflection of the timing of the switch, where companies with greater agency costs elected to not follow AA simply because they were unaware of which firm the AA team would join. Therefore, we allow companies within a follow designation to vary with the timing of the switch. We employ multinomial logistic regression that distinguishes between following or not, as well as whether a client selected a new audit firm before or after AA's conviction date.[19] If our results are a manifestation of the timing of the switch, then we expect the non-follow companies to be more likely to change auditors in the pre-conviction period. Alternatively, if our results extend beyond the timing of auditor changes studied in Barton (2005) and Chang et al. (2003), then we expect no systematic differences in the pattern of changing auditors pre- and post-conviction across the follow and non-follow groups.[20]

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[17] Standardization refers to subtracting the mean and scaling by the standard deviation of the variable in question, so that all variables have means equal to 0 and standard deviations of 1.

[18] We also estimated separate agency and switching costs regressions utilizing only those variables that were consistent with agency and switching costs, respectively. The adjusted R^2s from these regressions were 0.10 and 0.11, respectively, again indicating the two effects are approximately equal in our setting.

[19] We appreciate the suggestion by an anonymous referee to perform this analysis.

[20] The use of the conviction date to segregate the sample is admittedly arbitrary, but represents a date on which all sample companies knew they would have to change auditors and by which time a majority of the AA offices knew which audit firms they were joining.

Multinomial logit extends the binary logit model to multiple choices, and estimates the probability of a particular alternative relative to the probabilities of all other alternatives. In the current analysis, we utilize four categories: (1) non-follow companies that switched prior to the conviction, *NON-FOLLOW PRE* (n = 134); (2) non-follow companies that switched after the conviction, *NON-FOLLOW POST* (n = 47); (3) follow companies that switched prior to the conviction, *FOLLOW PRE* (n = 153); and (4) follow companies that switched after the conviction, *FOLLOW POST* (n = 73). The multinomial analysis conducted in Table 4 utilizes the *NON-FOLLOW PRE* companies as the comparison group for the other groups. The model provides the probabilities of being in the non-reference category (i.e., a positive coefficient indicates the company is more likely to be in the category indicated by the model rather than the *NON-FOLLOW PRE* category) while utilizing the information provided by all the categories.

Coefficient estimates and p-values for the multinomial logistic regression are presented in Table 4, columns 1 thru 6, while the last column provides tests of differences in the coefficients across the *FOLLOW PRE* and *POST* categories. Table 4, columns 5 and 6, illustrate that the *NON-FOLLOW PRE* and *POST* companies differ only on *SIZE* and *INSIDER*. Similarly, the last column illustrates that *SIZE* is the only significantly different factor across the *FOLLOW PRE* and *POST* groups. These results are consistent with Barton (2005), which finds that larger companies tended to change auditors earlier after the collapse of Enron. However, the fact that the companies that switched prior to the conviction are not significantly different across the *SIZE* dimension (coefficient estimate 0.00, p-value 0.98) indicates our follow designation is not simply a manifestation of the timing of the switch. Further, the lack of other significant differences *within* the follow and non-follow groups indicates the Table 3 results are not attributable to the timing of the switch.

The Table 4 findings further explain some of the results observed in Table 3. For instance, the significance of the coefficients on *CLIENTS* and *BLOCK* is primarily related to the *FOLLOW PRE* group. Further, the coefficient on *ACCT_FE* approaches marginal significance (p-value 0.11) for the *FOLLOW PRE* companies with untabulated results illustrating a significant difference between the *FOLLOW PRE* and *NON-FOLLOW POST* categories (p-value 0.02). Finally, while only the *FOLLOW POST* companies have significantly greater *TRANSPARENCY* than the *NON-FOLLOW PRE* companies (p-value 0.02), untabulated results find that both *FOLLOW* groups have significantly greater *TRANSPARENCY* than the *NON-FOLLOW POST* group (p-values 0.03 and 0.00, for the *FOLLOW PRE* and *POST* categories, respectively). Overall, the results in Table 4 are consistent with Table 3 and help illustrate that switching costs played a role in determining the selection of a new auditor after the collapse of AA regardless of the timing of the switch.

Robustness Tests

In this section, we summarize the results of several sensitivity tests that examine the robustness of our primary results in Tables 3 and 4.

Alternative industry definitions. Several of the variables used in our models (*FEE_EXPERT*, *CLIENTS*, *TRANSPARENCY*, *ACCRUAL*, *HIGHEST*, *LOWEST*, and industry-fixed effects) are a function of industry definitions. Reported results throughout the paper are based on industries as defined in Barth et al. (1998). We investigated the sensitivity of our results to using three alternative industry definitions: two-digit SIC codes, industry groupings in Fama and French (1997), and Francis et al. (1999), which resulted in 54, 44, and 27 industry groupings for our sample, respectively. Repeating our tests from Tables 3 and 4 using each alternative and re-estimating all variables requiring industry classifications,

TABLE 4
Multinomial Logistic Regression of the Follow Decision Pre- versus Post-Conviction Date

	FOLLOW				*NON-FOLLOW*		*FOLLOW*
	Pre-Conviction Date[b] (*PRE*)		Post-Conviction Date[c] (*POST*)		Post-Conviction Date[c] (*POST*)		*PRE*[b] versus *POST*[c]
Variable[a]	**Coeff. Est**	**p-value**[d]	**Coeff. Est**	**p-value**[d]	**Coeff. Est**	**p-value**[d]	**p-value**[e]
FEE_EXPERT	0.04	0.91	−0.22	0.61	−0.09	0.85	0.54
CLIENTS	1.39	0.00	0.79	0.22	−0.15	0.84	0.30
TENURE	−0.01	0.72	−0.03	0.23	0.01	0.69	0.34
SIZE	0.00	0.98	−0.40	0.00	−0.47	0.00	0.00
TRANSPARENCY	0.07	0.18	0.14	0.02	−0.10	0.19	0.19
COMPLEX	−0.64	0.05	−0.94	0.03	−0.72	0.16	0.48
ACCRUAL	4.41	0.00	3.92	0.00	2.90	0.07	0.71
INSIDER	0.72	0.05	0.47	0.28	1.14	0.01	0.53
LEVERAGE	1.04	0.17	0.41	0.67	1.29	0.26	0.47
BLOCK	−0.82	0.03	−0.57	0.17	−0.53	0.29	0.56
INDAUDIT	−0.47	0.19	−0.25	0.60	0.13	0.84	0.62
ACCT_FE	0.44	0.11	−0.06	0.86	−0.52	0.21	0.13
ROA	0.33	0.35	0.46	0.29	0.56	0.39	0.78
LOSS	−0.32	0.33	−0.66	0.10	−0.36	0.45	0.37
Pseudo R^2	0.37						

(*continued on next page*)

TABLE 4 (continued)

n *FOLLOW PRE*	153
n *FOLLOW POST*	73
n *NON-FOLLOW PRE*	134
n *NON-FOLLOW POST*	47

This table presents results from a single multinomial logistic regression with the sample of Non-Follow companies that switched prior to AA's conviction on June 15, 2002 (*NON-FOLLOW PRE*) serving as the reference category. The model includes unreported industry-fixed effects.

All p-values are two-tailed.

Refer to Figure 1 for hypotheses' sign predictions.

[a] Variable Definitions:

FOLLOW = 1 if a client is designated as following their former AA audit team to a new auditor, 0 otherwise;
FEE_EXPERT = 1 if AA had the greatest total audit fees in an industry and state, 0 otherwise;
CLIENTS = 1 if AA had the greatest number of clients in an industry and state, 0 otherwise;
TENURE = the number of years audited by AA per Compustat;
SIZE = natural logarithm of total assets (data6);
TRANSPARENCY = descending rank of the absolute value of the residual from a cross-sectional regression of annual returns on *ROA*, changes in earnings, *SIZE*, and industry-fixed effects;
COMPLEX = geographic sales diversity of a company;
ACCRUAL = performance-matched discretionary accruals utilizing the modified Jones (1991) model and adjusting by the median discretionary accruals for companies in the same industry and *ROA* decile;
INSIDER = 1 if an insider has 5 percent or more of the stock per Spectrum, 0 otherwise;
LEVERAGE = total debt divided by total assets;
BLOCK = 1 if an outside blockholder has 5 percent or more of the stock per Spectrum, 0 otherwise;
INDAUDIT = 1 if the audit committee responsible for making the follow decision was 100 percent independent, 0 otherwise;
ACCT_FE = 1 if the audit committee has an accounting financial expert, 0 otherwise;
ROA = net income before extraordinary items divided by ending total assets; and
LOSS = 1 if *ROA* is less than 0, 0 otherwise.

[b] Pre-Conviction Date designates those companies that switched prior to AA's conviction on June 15, 2002 (*PRE*).

[c] Post-Conviction Date designates those companies that switched after AA's conviction on June 15, 2002 (*POST*).

[d] Reported p-values are for the reported coefficient estimates.

[e] Reported p-values are for the indicated tests of differences in reported coefficient estimates.

our inferences remained unchanged. However, some industry definitions (i.e., two-digit SIC codes and Fama and French [1997]) result in quasi-complete separation of the data in our Tables 3 and 4 because of the increased number of industry control variables required by these definitions coupled with the small sample size.

Alternative definitions of auditor expertise. Revising the definitions of auditor expertise by requiring AA to have at least 10 percent more audit fees or clients than the next closest competitor in that state and industry does not change our inferences (p-values of 0.84 and 0.01 for *FEE_EXPERT* and *CLIENTS*, respectively in Table 3 analysis, and in Table 4 analysis only the coefficient on *CLIENTS* in the *PRE-FOLLOW* category is significant, p-value of 0.01). We also re-estimated *FEE_EXPERT* and *CLIENTS* on a city-level basis according to the methodology in Francis, Reichelt, and Wang (2005), which utilizes two-digit SIC codes and the U.S. Census Bureau's metropolitan statistical areas. When included in the tests in Tables 3 and 4, the coefficients on the city-level variables were not significantly different from zero, regardless of the industry definition utilized (all p-values > 0.15).[21] Approximately 20 percent of our sample companies experienced a switch in their audit opinion cities after the collapse of AA, implying that city-level measures of expertise are not capable of capturing the competitive landscape for a significant proportion of our sample.

Finally, given the magnitude of the effect of *CLIENTS* on the follow decision documented in Tables 3 and 4, we re-estimated the models excluding this variable. The inferences remain unchanged and the model is still well specified as indicated by the model fit and discrimination statistics (Hosmer and Lemeshow p-value of 0.12, and the ROC Curve statistic of 0.71).

Alternative definitions of *COMPLEX*. Next, we tested the sensitivity of our measure of company and audit complexity, *COMPLEX*. We supplemented the models in Tables 3 and 4 with three alternative measures suggested by prior research (Simunic and Stein 1987): total number of geographic segments, total number of business segments, and a measure equivalent to *COMPLEX* that utilizes business segments rather than geographic segments. In untabulated results, none of the alternatives was incrementally significant (p-values of 0.29, 0.38, 0.56, respectively), nor did their inclusion qualitatively alter any of the reported results.

Additional proxies for agency costs. Prior research on the association between audit quality and agency benefits has included a number of proxies for agency costs (e.g., DeFond 1992; Francis and Wilson 1988). To test the robustness of our findings, we expanded the models in Tables 3 and 4 to include three additional proxies: the need for external financing using Kaplan and Zingales (1997), stock price volatility for the calendar year 2001, and institutional holdings. When the variables were included in the model individually or as a group, the coefficients on each of the additional proxies were not significantly different from zero (p-values of 0.67, 0.23, 0.31 for individual tests, and 0.56, 0.21, 0.34 when included at the same time, respectively) and our inferences remain unaltered.

To augment our agency hypothesis tests, we collected information concerning board of director characteristics commonly used in corporate governance research, including the percentage of independent directors, total number of directors, and whether the Chairman of the Board is also an employee of the company. When added to the models in Tables 3 and 4, none of the additional corporate governance variables was significant (p-values of 0.54, 0.80, 0.65, respectively), nor did they qualitatively alter any of the reported results.

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[21] Francis, Reichelt, and Wang (2005) notes that their results are robust to the Barth et al. (1998) industry definitions.

Other sensitivity tests. No change in inferences resulted when we repeated the tests in Tables 3 and 4 and included AA office-fixed effects instead of industry-fixed effects. Furthermore, the inclusion of company-specific, three-day market model abnormal returns surrounding AA's indictment date is not significant (p-value 0.16) and has no effect on any of the reported results. This suggests that the market reaction on the indictment date was a reflection of both agency and switching costs for sample companies. Finally, the Table 3 and 4 results are not sensitive to (1) excluding all observations that switched prior to the announcement of their AA office takeover by another Big 4 audit firm (our primary mechanism for determining the follow designation) and, (2) coding all of these same observations as non-follow regardless of the audit firm they eventually selected.

IV. FINANCIAL STATEMENT QUALITY

Tension between agency benefits and switching costs is at the heart of the debate on mandatory auditor rotation. Proponents of mandatory auditor rotation argue that financial reporting will be improved by forcing companies to periodically change auditors, thereby resulting in agency benefits. In an effort to examine this issue, a number of studies have investigated the relation between auditor tenure and audit/earnings quality, with mixed results. Deis and Giroux (1992) analyzes a sample of small CPA firms auditing independent school districts and found a reduction in audit quality (defined as the probability of detecting and reporting a breach in the client's accounting system) with increased tenure. More recently, Myers et al. (2003) finds a positive relation between auditor tenure and the quality of earnings measured as the absolute value of discretionary accruals. They interpret their findings as being *inconsistent* with mandatory auditor rotation improving financial reporting.

The forced change for AA clients has the potential to be incrementally informative for this debate. Nagy (2005) finds that abnormal accruals were lower in 2002 and 2003 as compared to 2000 and 2001 for all Big 4 audit clients and incrementally lower for former AA clients. He attributes the decline to increased skepticism by the successor auditor. Cahan and Zhang (2006) find that former AA clients had lower levels of abnormal accruals in 2002 relative to other companies audited by the Big 4. They attribute more conservative accounting to the successor auditor compensating for an actual or perceived higher litigation risk for former AA clients. These results suggest that the forced change may have improved financial reporting. However, neither study differentiates companies based on the follow decision. Because financial statements and reported accruals are jointly determined by the client and auditor, our analysis, which considers the client's choice of auditor, provides additional insights on this matter.

Research Design

We expand the discretionary accrual model in Myers et al. (2003) to include our *FOLLOW* variable and indicators for extreme *ACCRUAL* quintiles:[22]

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[22] An additional distinction between our analysis and Myers et al. (2003) is that we adjust discretionary accruals for performance. Given our sample size and our control/treatment research design, performance-adjusted discretionary accruals are the most appropriate measures of aggressive behavior in this context (see Kothari et al. 2005).

$$\begin{aligned} ACCRUAL = {} & \textstyle\sum_I \alpha_I + \beta_1 FOLLOW + \beta_2 LOWEST + \beta FOLLOW{*}LOWEST \\ & + \beta_4 HIGHEST + \beta_5 FOLLOW{*}HIGHEST + \beta_6 TENSURE \\ & + \beta_7 AGE + \beta_8 SIZE + \beta_9 INDUSTRYGROWTH \\ & + \beta_{10} CASHFLOW + \varepsilon \end{aligned} \tag{4}$$

where *ACCRUAL*, *FOLLOW*, *TENURE*, *SIZE*, and industry indicator variables are as defined previously. The remaining variables are defined as follows (Compustat data items in parentheses):

LOWEST = 1 if *ACCRUAL* is in the lowest quintile during last year audited by AA, 0 otherwise;
HIGHEST = 1 if *ACCRUAL* is in the highest quintile during last year audited by AA, 0 otherwise;
AGE = number of years for which total assets (#6) was reported in Compustat since 1980;
INDUSTRYGROWTH = $\sum_{i=1}^{N} Sales_{i,t} / \sum_{i=1}^{N} Sales_{i,t-1}$ by industry; and
CASHFLOW = cash flow from operations (#308) divided by ending total assets (#6).

LOWEST and *HIGHEST* distinguish companies in the lowest and highest quintiles of *ACCRUAL* as of the last year audited by AA (i.e., companies in the highest [lowest] quintile in the last year audited by AA, year *t*, are also coded as highest [lowest] in *t*+1).

We allow the coefficients on the extreme quintiles to vary with *FOLLOW* in order to determine whether discretionary accrual behavior is associated with the decision to sever ties with the AA team. Given that non-follow companies clearly have a new auditor, we expect extreme quintile companies from this sample to have a higher probability of exhibiting reversion behavior (i.e., the coefficients on *LOWEST* and *HIGHEST* are expected to be insignificantly different from zero in the first year of the new auditor). We do not make predictions for the corresponding follow companies since they have essentially only changed the name of their auditor rather than the underlying relationship.

Results

Results are reported in Table 5 for the final year audited by AA (year *t*) and the first year audited by the new auditor (year *t*+1).[23] Consistent with Myers et al. (2003), *INDUSTRYGROWTH* and *CASHFLOW* are significantly positive and negative, respectively. However, contrary to the findings in Myers et al. (2003), *TENURE*, *AGE*, and *SIZE* are insignificant. The lack of significance is likely attributable to our limited sample size reducing the cross-sectional variation in the estimates.[24]

The insignificance of the *FOLLOW* variable suggests that the middle three quintiles of the *ACCRUAL* variable are not significantly different on average from the corresponding group of non-follow companies in either year. Next, as indicated by the negative coefficient

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[23] By design, *HIGHEST* and *LOWEST* are significantly different from zero in year *t*. This prohibits comparisons of the coefficients across time and explains the relatively high R^2 in year *t* versus year *t*+1.

[24] In contrast to our sample of 407 companies, Myers et al. (2003) utilize all observations on Compustat with the requisite data yielding 41,250 observations.

TABLE 5
Regressions of Performance-Adjusted Discretionary Accruals on the Follow Decision and Control Variables

$$ACCRUAL = \sum_{l}\alpha_l + \beta_1 FOLLOW + \beta_2 LOWEST + \beta_3 FOLLOW{*}LOWEST + \beta_4 HIGHEST + \beta_5 FOLLOW{*}HIGHEST + \beta_6 TENURE + \beta_7 AGE + \beta_8 SIZE + \beta_9 INDUSTRYGROWTH + \beta_{10} CASHFLOW + \varepsilon$$

	Year *t*		Year *t*+1	
Variable[a]	**Coeff. Est.**	**p-value**	**Coeff. Est.**	**p-value**
FOLLOW	0.01	0.55	0.02	0.13
LOWEST	−0.17	0.00	−0.04	0.02
*FOLLOW*LOWEST*	0.03	0.12	0.00	0.93
HIGHEST	0.15	0.00	0.05	0.02
*FOLLOW*HIGHEST*	0.00	0.87	−0.06	0.02
TENURE	0.00	0.53	−0.01	0.58
AGE	0.00	0.57	0.00	0.28
SIZE	0.00	0.84	−0.01	0.08
INDUSTRY GROWTH	0.32	0.00	−0.08	0.55
CASH FLOW	−0.16	0.00	−0.32	0.00
$\beta_2 + \beta_3$	−0.14	0.00	−0.04	0.02
$\beta_4 + \beta_5$	0.15	0.00	−0.01	0.47
n Follow	226		226	
n Non-Follow	181		181	
Adj. R^2	0.75		0.31	

This table presents regressions of performance-adjusted discretionary accruals in the final year audited by AA (year *t*) and the first year audited by the new auditor (year *t*+1). Companies are classified as being in the lowest or highest performance-adjusted accrual quintile in year *t*.

Reported p-values are based on two-tailed tests.

The model includes unreported industry-fixed effects.

[a] Variable Definitions:

ACCRUAL = performance-matched discretionary accruals utilizing the modified Jones (1991) model and adjusting by the median discretionary accruals for companies in the same industry and *ROA* decile;
FOLLOW = 1 if a client is designated as following their former AA audit team to a new auditor, 0 otherwise;
LOWEST = 1 if *ACCRUAL* in year *t* was in the lowest quintile, 0 otherwise;
*FOLLOW*LOWEST* = interaction of *FOLLOW* and *LOWEST*;
HIGHEST = 1 if *ACCRUAL* in year *t* is in the highest quintile, 0 otherwise;
*FOLLOW*HIGHEST* = interaction of *FOLLOW* and *HIGHEST*;
TENURE = number of years audited by AA per Compustat;
AGE = number of years the company reported total assets on Compustat since 1980;
SIZE = natural logarithm of total assets;
INDUSTRY GROWTH = total industry sales in the current year divided by total industry sales in the prior year, where industries are defined as in Table 1; and
CASH FLOW = cash flow from operations at the end of the indicated year divided by ending total assets.

on *LOWEST*, non-follow companies in the extreme negative *ACCRUAL* quintile had persistently lower discretionary accruals than the remainder of the sample in both years analyzed. More importantly, the *FOLLOW* companies do not appear to behave differently after the change in auditors relative to the non-follow companies, as witnessed by the insignificance of the coefficient on *FOLLOW*LOWEST*.

The *HIGHEST* companies also exhibit some persistence with non-follow companies being different on average in both the final year with AA (year *t*) and the first year audited by the new auditor (year *t*+1). This suggests that the forced auditor change did not serve to rein in this relatively aggressive behavior. The *FOLLOW* companies in the highest quintile, on the other hand, are no longer significantly different on average from the middle three quintiles, implying that their relatively high accrual behavior in the final year of AA was not repeated under the new audit firms.[25]

Overall, we find evidence that companies chose to follow (not follow) AA if they had higher (lower) discretionary accruals. We find no evidence that accrual behavior improved for follow or non-follow clients in the lowest quintile of discretionary accruals. Further, there is no evidence that *NON-FOLLOW* companies in the highest discretionary accrual quintile curbed their discretionary accruals after selecting an entirely new auditor. In contrast, *FOLLOW* companies in the same category no longer exhibited higher performance-adjusted discretionary accruals on average after following AA to a new auditor. Combined, this evidence does not support the contention that mandatory auditor rotation would necessarily improve financial reporting, confirming conclusions reached in Myers et al. (2003).

Ex post, there are several possible causes of the unexpected result for the companies in the highest performance-adjusted discretionary accrual quintile. First, AA partners moving to a new auditor may have been more likely to rein in discretionary accruals given their reduction in wealth and other disutilities while at AA. Second, audit firms taking on AA *clients and personnel* may have subjected the companies to increased levels of scrutiny because of the Enron and WorldCom fiascos or perceived higher litigation risk. Discussions with audit firm partners, both formerly from AA and those who took on AA clients, fail to confirm this latter conjecture, though we have no way of empirically validating this. Finally, as documented in Tables 3 and 4, companies with greater agency concerns are more likely to be non-follow companies, which is consistent with changing auditors in an effort to signal their accrual quality to the market by having new independent auditors opine on the relatively high discretionary accrual behavior.

Robustness Tests

The multinomial tests from Table 4 find that, relative to the pre-conviction non-follow group, all other companies had significantly higher performance-adjusted discretionary accruals on average. Further, companies from the post-conviction period were smaller companies that might exhibit significantly different accrual behaviors. In order to assess the sensitivity of the Table 5 results to the composition of the companies in the highest quintile, we examined the proportion of follow and non-follow companies within the *HIGHEST* category, along with separating them into pre- and post-conviction categories. The results reveal that the relative proportion of follow and non-follow companies and of pre- and post-conviction companies are not statistically different from those observed in the full sample in Table 4. This alleviates concerns that the timing of the switch influenced the accrual results. Also, we re-estimated the analysis in Table 5 utilizing only the pre-conviction groups, and the results hold with only the *HIGHEST* follow companies exhibiting reversion in their behavior, which provides further assurance that the results in Table 5 are not due to differences in the timing of auditor changes.[26]

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[25] The incremental coefficient for companies that followed and were in the high accrual quintile is *HIGHEST*+*FOLLOW*HIGHEST*. Reported statistics are included in the bottom of Table 5.

In addition, our Table 5 results are not sensitive to alternative industry definitions including two-digit SIC codes, Fama and French (1997), and Francis et al. (1999). Furthermore, we repeated the Table 5 analyses to include AA office fixed-effects as well as utilizing non-performance-matched discretionary accruals with no change in inferences.

V. CONCLUSION

The AA collapse presents a rare opportunity to study the determinants of auditor selection. Ordinarily, researchers are limited to switching decisions that are created by an auditor resignation or client dismissal. Both are events potentially contaminated by other information contained in the decision to change auditors. In the current setting, all AA clients had to find new auditors, thereby mitigating any signaling issues related to the dismissal of AA. We contribute to the auditor change literature by adopting a different methodology that allows us to focus on factors involved in the selection of a new auditor, namely switching and agency costs. We view this methodology and our results as a significant contribution to the literature.

The results indicate that companies consider both switching and agency costs in selecting a new auditor. We find that companies with the most aggressive accruals, with an accounting financial expert on their audit committee and where AA was the industry leader were more likely to follow AA. As such, for some companies the cost of switching auditors outweighed any agency benefits forgone by following AA. On the other side of the trade-off, we find that companies with higher agency concerns, as captured by the existence of an outside blockholder, low financial reporting transparency, and greater geographic diversity, were more likely to sever ties with AA and start a completely new audit firm relationship. This suggests the agency costs borne by following AA outweighed the benefits of reduced switching costs. It also suggests that companies for whom agency concerns are the most acute consider the independence of their auditor, in fact and appearance, in mitigating these costs.

In addition, we find that the companies in the highest quintile of performance-matched discretionary accruals that followed AA curbed their accrual behavior in the year after AA's collapse, while there was no change for those that did not follow AA. This suggests that the mandatory rotation of auditors may not improve financial reporting.

Overall, we conclude that companies trade-off both agency and switching costs in the selection of a new auditor. We interpret this evidence as being consistent with the notion that switching costs in non-forced auditor change settings likely outweigh the agency benefits of changing auditors in many cases, which is consistent with the infrequency of auditor changes for most companies. In our forced auditor change setting, the results illustrate that more complex/less transparent companies do perceive there to be agency benefits to changing auditors but, in many instances, these benefits are not likely to outweigh the savings from maintaining their current audit personnel. Finally, our findings suggest that a mandatory auditor rotation regime would not necessarily improve earnings quality. Our results should be of interest to regulators, standard-setters, and academics who are debating the efficacy of the Sarbanes-Oxley Act of 2002 and mandatory auditor rotation, as well as to those interested in the factors involved in the selection of a new auditor.

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[26] We also examined the relative proportions of the remaining Big 4 audit firms represented in the *HIGHEST* category.. The results in Cahan and Zhang (2006) indicate only former AA clients that subsequently hired Ernst & Young (EY) experienced significantly lower levels of abnormal accruals. In our sample, the relative proportion of EY clients in the *HIGHEST* portfolio is 32 and 25 percent for the follow and non-follow groups, respectively, indicating the reversal for the follow group is not simply a manifestation of the EY effect documented in Cahan and Zhang (2006).

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THE ACCOUNTING REVIEW
Vol. 82, No. 3
2007
pp. 651–677

The Influence of Forecast Dispersion on the Incremental Explanatory Power of Earnings, Book Value, and Analyst Forecasts on Market Prices

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ABSTRACT: This study investigates the influence of analyst forecast dispersion on Ohlson's (2001) proposed linear information dynamics where consensus analyst forecasts are suggested as a proxy for *other* information. Our results indicate that Ohlson's proposed valuation model is most descriptive of market pricing when forecast dispersion, and hence information asymmetry, is high. Our results also suggest that when analysts are confronted with high information asymmetry, they tend to focus less on accounting fundamentals and rely more on other nonaccounting information, thus decreasing the correlation between the explanatory power of analyst forecasts and that of earnings and book value.

Keywords: *analyst forecasts; analyst forecast dispersion; explanatory power.*

Data Availability: *Data is available from the sources listed in the text.*

I. INTRODUCTION

Ohlson (1995) models firm value as a linear function of earnings, book value, and other unspecified information, v_t. The unspecified nature of the other information in this model precludes direct empirical validation of the model's information dynamics. Ohlson (2001) argues that v_t plays an important role in the linear information dynamics and proposes consensus analyst forecasts as a proxy for v_t.

We acknowledge the workshop participants at Georgia State University, Lehigh University, University of Oklahoma, State University of New York at Buffalo, Syracuse University, and Virginia Commonwealth University, along with the helpful comments of Anwer Ahmed, Larry Brown, Steve Ciccone, Patricia Dechow, Peter Easton, Carol Ann Frost, Susan Hamlen, Jerry Han, David Harris, Troy Janes, Kenneth Kim, Bob Lipe, Carol Liu, Steve Matsunaga, Dale Morse, Arianna Pinello, Doug Stevens, Wayne Thomas, Clark Wheatley, Jeff Wong, Huai Zhang, and two anonymous reviewers, along with the research assistance of Xinrong Qiang and Lauren Vigiano. We gratefully acknowledge the contribution of I/B/E/S Inc. for providing earnings per share forecast data, available through the Institutional Brokers Estimate System. I/B/E/S provides this data as part of a broad academic program to encourage earnings expectation research. This paper was formerly titled, "Empirical Implications of Analyst Forecast Dispersion to the Information Dynamics of Valuation Models: A Two Stage Approach."

Editor's note: This paper was accepted by Patricia Dechow.

Submitted November 2004
Accepted September 2006

Ohlson's (2001) inclusion of analyst forecasts presumes that analysts convey information to the market beyond that reflected by the accounting fundamentals of earnings and book value. Dechow et al. (1999) indicate that Ohlson's proposed model also presumes that earnings and book value reflect information not captured in analyst forecasts. Together, these two conditions imply that Ohlson's model would be most descriptive of market pricing when analyst forecasts do not fully capture the information reflected by earnings and book value, but capture "other information" beyond that reflected by the accounting fundamentals.

Dechow et al. (1999) test Ohlson's (2001) proposed model using one-year-ahead consensus analyst forecasts to proxy for v_t and find that market prices are associated with analyst forecasts *incrementally* to the association of market prices with the accounting fundamentals, but also find that Ohlson's proposed model is no more descriptive of market pricing than is a naïve model that capitalizes one-year-ahead analyst forecasts. Consensus analyst forecasts, therefore, fully capture on average the information reflected by earnings and book value, as expected with generally efficient markets. We posit that when the information environment is poor, however, it is unlikely for any summary measure to be a sufficient statistic for firm value.

In this study, our initial analysis validates Dechow et al.'s (1999) cross-sectional findings. Further analysis reveals, however, that when analyst forecast dispersion, and hence information asymmetry, is high, Ohlson's (2001) proposed model has greater explanatory power than the naïve model. These results suggest that in poor information environments where earnings quality is also poor, analysts are forced to focus less on accounting fundamentals and more on "other" relevant information beyond that reflected by the accounting fundamentals.

The next section develops our empirical model and presents our hypotheses. Section III describes the sample selection procedure and the empirical data. Our tests of hypotheses are reported in Section IV. Finally, Section V presents our conclusions.

II. MODEL AND HYPOTHESES DEVELOPMENT

We base our tests of hypotheses on Ohlson (1995, 2001) who models market value as a function of earnings, book value, and other information, as summarized below (see Hand 2001):[1]

$$P_{j,t} = \frac{R\omega}{R-\omega} X_{j,t} + \frac{(R-1)\omega}{R-\omega} BV_{j,t} + \frac{R}{(R-\omega)(R-\gamma)} v_{j,t} + \varepsilon_{j,t} \tag{1}$$

where:

$P_{j,t}$ = market value (price) of equity of firm j at time t;
$X_{j,t}$ = firm j's earnings, from time $(t-1)$ to t;
$BV_{j,t}$ = firm j's net book value of equity at time t;

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[1] Ohlson (1995) also incorporates net dividends into his model. For simplicity and to be consistent with Dechow et al. (1999), we assume zero dividends in our analysis. and omit dividends from our empirical tests. The exclusion of dividends does not alter our hypotheses or results in any material way. As an alternative, Dechow et al. (1999) tested the residual income valuation (RIV) model where firm value is a function of book value, earnings beyond a normal rate of return (abnormal earnings, $X^a_{j,t}$), and $v_{j,t}$. The RIV model is mathematically equivalent to Equation (1), but Ohlson (2001) points out that the abnormal earnings framework is not crucial to his analysis and emphasizes the earnings-based model to empiricists. As a test of sensitivity, we also perform our tests of hypotheses using the RIV model and find essentially identical results.

R = 1 plus the cost of capital (r);
ω = persistence of earnings beyond a "normal" rate of return;
γ = persistence of "other information"; and
$\varepsilon_{j,t}$ = residual.

Ohlson (2001, 112–113) proposes setting $\nu_{j,t}$ to "analysts' consensus forecasts of next-year's earnings." He argues that analyst forecasts ($\overline{X}^{t+1}_{j,t}$) contain information representing investors' current expectation of future earnings. Analyst forecasts, however, depend in part on the information contained in current earnings and book value. Analyst forecasts, therefore, not only reflect information about future earnings *beyond* that conveyed by earnings and book value (the definition of ν_t), but also reflect the "stale" information *concurrently* conveyed by the accounting fundamentals. $\overline{X}^{t+1}_{j,t}$ will, therefore, be highly correlated with $X_{j,t}$ such that the empirical implications from regressing Equation (1) inclusive of $\overline{X}^{t+1}_{j,t}$ would be garbled.

Ohlson (2001) illustrates this garbling through his prediction that the valuation coefficient on $X_{j,t}$ would be $-r\omega\gamma/(R-\omega)(R-\gamma)$. Empirically, the predicted negative coefficient on $X_{j,t}$ is not directly interpretable in a valuation context. The garbling, however, does not affect the predicted coefficient on $\overline{X}^{t+1}_{j,t}$, which is exactly as stated in Equation (1) above.

We avoid this garbling problem by estimating $\nu_{j,t}$ in a first stage regression. Since $\nu_{j,t}$ equals $\overline{X}^{t+1}_{j,t} - \omega(1+R)X_{j,t} - R(1-\omega)BV_{j,t}$, we estimate $\nu_{j,t}$ directly from the accounting fundamentals, as follows:

$$f_{j,t} = \delta_0 + \delta_1 X_{j,t} + \delta_2 BV_{j,t} + \nu_{j,t} \tag{2}$$

where:

$f_{j,t}$ = consensus analyst forecasts of next year's earnings;
δ_i = regression coefficients, i$\in$(0, 2); and
$\nu_{j,t}$ = regression residual, which proxies for "other information" in the second stage.

For each sample year, we estimate Equation (2) using all prior sample-year observations. This two-stage approach parallels Ohlson and Shroff's (1992) approach for identifying new information in reported earnings (i.e., unexpected earnings) and Manry et al.'s (2003) approach for identifying new information in reported quarterly earnings. $\hat{\nu}_{j,t}$ serves as our proxy for other information in Equation (1) for the second stage, as follows:[2]

$$P_{j,t} = \beta_0 + \beta_1 X_{j,t} + \beta_2 BV_{j,t} + \beta_3 \hat{\nu}_{j,t} + \mu_{j,t} \tag{3}$$

where:

β_i = regression coefficients, $i \in (0, 3)$;
$\hat{\nu}_{j,t}$ = Equation (2) residual and proxy for "other information" in $f_{j,t}$; and
$\mu_{j,t}$ = regression residual.

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[2] In contrast, Dechow et al. (1999) in using the RIV model avoid the garbling problem by estimating the abnormal component of analyst forecasts. They calculate this abnormal component by subtracting the persistent part of abnormal earnings from analyst forecasts, where persistence (ω) is estimated in a first-stage regression. While the models in Dechow et al. (1999) and our study are theoretically equivalent, their estimation procedure formally imposes the restrictive assumptions of Ohlson's information dynamics (i.e., $0 \leq \omega \leq 1$). Our estimation procedure relaxes this assumption and allows parameter values to be estimated empirically.

Equation (3) serves as our representation of Ohlson's (2001) proposed model, consisting of earnings, book value, and analyst forecast residuals (hereafter, the EBAF model). If analyst forecasts convey information beyond that reflected by earnings and book value alone, we would expect to observe not only a positive coefficient on $\hat{v}_{j,t}$ (i.e., $\beta_3 > 0$), we would also expect the overall explanatory power of Equation (3) to be greater than the explanatory power of a model that excludes "other information" from the regression. As a basis of comparison, therefore, we create a baseline model that suppresses the other information term in Equation (3) and simply regress market prices on earnings and book value (hereafter, the EB model). We test whether the inclusion of analyst forecast residuals improves the explanatory power of Ohlson's valuation model relative to the explanatory power of earnings and book value alone by comparing the adjusted R^2s of the EBAF model (R^2_{EBAF}) and the EB model (R^2_{EB}). This test is formally stated (in alternative form) as part of our first hypothesis, below:[3]

H1a (Explanatory Power of Analyst Forecasts Hypothesis): The adjusted R^2 of the earnings, book value, and analyst forecast residual (EBAF) model is significantly greater than the adjusted R^2 of the earnings and book value (EB) model ($R^2_{EBAF} - R^2_{EB} > 0$).

Dechow et al.'s (1999) findings indicate, however, that Ohlson's (2001) proposed model is no more descriptive of market pricing than is a naïve model that capitalizes analyst forecasts. The implication, therefore, is that earnings and book value have no incremental explanatory power relative to that of analyst forecasts. To test this implication, we create a new model that regresses prices on analyst forecasts, as follows:

$$P_{j,t} = \pi_0 + \pi_1 f_{j,t} + \kappa_{j,t} \tag{4}$$

where:

π_i = regression coefficients, $i \in (0, 1)$; and
$\kappa_{j,t}$ = regression residual.

In Equation (4) above (hereafter, the AF model), we allow the capitalization rate to be impounded directly into the regression coefficient, rather than dividing the independent variable $f_{j,t}$ by an assumed capitalization rate. Comparison of the adjusted R^2 of the AF model (R^2_{AF}) to that of the EBAF model (R^2_{EBAF}) serves as our basis of testing the explanatory power of earnings and book value.[4] This test is formally stated (in alternative form) below:

H1b (Explanatory Power of Earnings and Book Value Hypothesis): The adjusted R^2 of the earnings, book value, and analyst forecast residual (EBAF) model is significantly greater than the adjusted R^2 of the analyst forecast (AF) model ($R^2_{EBAF} - R^2_{AF} > 0$).

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[3] We also performed a similar analysis directly on Ohlson's (2001) linear information dynamics, where one-year-ahead earnings serves as the dependent variable. Our pricing tests subsume this analysis, however, in that the perceived relations for the valuation model are assumed to first hold for the linear information dynamics.

[4] Comparing the adjusted R^2s across the EB and AF models would also provide evidence on whether the explanatory power of analyst forecasts exceeds that of earnings and book value. Comparison of the R^2s for the EB and AF models to the EBAF model, however, not only subsumes those tests, but also tests the incremental explanatory power of the models.

This first set of hypotheses serves to confirm Dechow et al.'s (1999) findings and to establish benchmarks for the explanatory power of analyst forecasts and of earnings and book value. Our next set of hypotheses tests whether those benchmarks differ when the information environment is poor.

Ohlson (1995, 2001) argues that the extent to which market prices are explained by current earnings depends, in part, on the relation between current earnings and future earnings. The predictive ability of earnings, therefore, ultimately determines the explanatory power of the valuation model, exclusive of "other information." In poor information environments, however, the predictive ability of earnings will be low, relative to good information environments (by definition). The predictive ability of analyst forecasts in poor information environments will also likely be low since other factors that analysts consider in their forecasts will also likely be noisy in poor information environments (see, for example, Brown et al. 1987). We therefore expect that the explanatory power of analyst forecasts and that of earnings and book value will be lower in poor information environments.

Further, in poor information environments, analysts are unlikely to rely on poor-quality earnings when predicting future earnings performance. In fact, Liu et al. (2004), using forecast dispersion as a proxy for the information environment, document that in a poor information environment, analyst forecasts are less correlated with earnings and book value. Liu et al.'s (2004) findings suggest that analysts in poor information environments will weight other possibly value-relevant factors more heavily relative to the weighting on earnings and book value in formulating their forecasts. In poor information environments, therefore, analyst forecasts will be based less on past earnings performance and more on other factors, thus increasing the proportion of the explanatory power of analyst forecasts that is uncorrelated with the explanatory power of earnings and book value.

If analysts shift away from earnings and book value as a basis for their forecasts in poor information environments, then the probability increases that earnings and book value contain relevant information (albeit noisy) about future earnings not captured by analysts. In poor environments, analysts are unlikely to invest additional resources to discern between noise and information in reported earnings since they have other value-relevant factors to consider. Other market participants without other sources of information will be more likely to invest in identifying the information reflected by poor-quality earnings. Thus, in poor information environments, we expect a higher proportion of the explanatory power of earnings and book value to be uncorrelated with analyst forecasts than in good information environments.

Before formally forming our next set of hypotheses, we must first create a proxy for the information environment. Among those measures that the prior literature has found to be effective proxies for the information environment, forecast dispersion is the most closely associated to our metric for $\nu_{j,t}$. Brown et al. (1987) and later Wiedman (1996) find that analyst forecast dispersion is an effective proxy for the noise in analyst forecasts, and Barron et al. (1998) find that forecast dispersion is strongly associated with a firm's level of information asymmetry and uncertainty.[5] Further, Gebhardt et al. (2001) and Han and Manry (2000) find that those firms exhibiting high forecast dispersion also exhibit high costs of capital and low earnings persistence.[6] We formally state our final set of hypotheses (in alternative form), below.

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[5] In Section IV, we report the results from using other proxies for the information environment.

H2a (**$H-L$: Explanatory Power of Analyst Forecasts Hypothesis**): The incremental explanatory power of analyst forecasts relative to earnings and book value is greater for high-dispersion firms than for low-dispersion firms ($(R^2_{EBAF} - R^2_{EB})^H > (R^2_{EBAF} - R^2_{EB})^L$ or $\Delta(R^2_{EBAF} - R^2_{EB}) > 0$).

H2b (**$H-L$: Explanatory Power of Earnings and Book Value Hypothesis**): The incremental explanatory power of earnings and book value relative to analyst forecasts is greater for high-dispersion firms than for low-dispersion firms ($(R^2_{EBAF} - R^2_{AF})^H > (R^2_{EBAF} - R^2_{AF})^L$ or $\Delta(R^2_{EBAF} - R^2_{AF}) > 0$).

III. SAMPLE SELECTION AND DATA DEFINITIONS

Sample Selection Criteria

Sample firms were identified by merging Compustat-listed firms with firms listed on I/B/E/S over the 1984 to 2003 period. This resulted in an initial sample size of 42,320 firm-years, excluding firms from regulated financial institutions and utilities, and firms with negative book value. As shown in Table 1, we deleted 348 observations with missing Compustat data on average shares outstanding, book value of equity, factor to adjust for stock splits or stock dividends, income before extraordinary items, market price, and preferred dividends, and 159 observations with missing I/B/E/S data on one-year-ahead consensus annual earnings forecasts. We also deleted 2,605 observations with negative consensus analyst forecasts, since our tests require the capitalization of consensus analyst forecasts and capitalizing negative forecasts would imply the existence of negative prices.

TABLE 1
Sample Selection

Descriptions	**Firms**
Firm-years listed on both Compustat and I/B/E/S from 1984 to 2003 (excluding financial institutions and utilities and firms with negative book value).	42,320
Less firm-years with:	
Missing Compustat data	(348)
Missing I/B/E/S data	(159)
Subtotal	41,813
Less firm-years with:	
Negative forecasts	(2,605)
Missing standard deviation of forecasts	(11,480)
Final Sample	27,728

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[6] More specific findings that also suggest that analyst forecast dispersion measures uncertainty and information asymmetry in the capital markets include evidence that forecast dispersion increases surrounding earnings announcements (Morse et al. 1991; Brown and Han 1992), option listings (Li-Chen et al. 1995), corporate restructurings (Lopez and Clement 2000), and changes in accounting methods (Elliott and Philbrick 1990). Other studies have shown that forecast dispersion increases with the level of discretionary accruals (Lee and Choi 2000) as well as when earnings are negative, decrease, or are volatile (Ciccone 2001; Ang and Ciccone 2001). Decreases in forecast dispersion have been documented to occur when the level of insider purchases increases (Lustgarten and Mande 1998) and when management discloses additional confirming forecasts (Clement et al. 2003).

Additional I/B/E/S data on the standard deviation of consensus annual earnings forecasts required forecasts from more than two analysts and resulted in an additional loss of 11,480 observations. This yielded a final sample of 27,728 firm-years over our 20-year sample period. To classify our sample into high and low forecast dispersion categories, we ranked our sample firms by year using the standard deviation of analyst forecasts, resulting in 13,875 observations being classified as high-dispersion firm-years and 13,853 observations being classified as low-dispersion firm-years.[7]

Data requirements of the additional sensitivity tests include the daily market returns from CRSP. From I/B/E/S, we required the adjusted reported earnings, long-term growth, and number of forecasts. We also required the standard industrial classification (SIC) code, total assets, and eight continuous years of income before extraordinary items from Compustat. The additional data requirements reduced our observations to 26,389 for our tests using returns, 26,075 for our tests conditioned on market beta, and 16,329 for our tests conditioned on earnings persistence.

Data Definitions

The dependent variable, market price ($P_{j,t}$), is set as the closing security price on the last trading day of the month in which we set our metric for consensus analyst forecasts, adjusted for stock splits and stock dividends. To control for heteroscedasticity and size, we follow Brown et al. (1999) and standardize both our dependent and independent variables by $P_{j,t-1}$.

Book value per share ($BV_{j,t}$), is total common equity divided by shares outstanding and earnings ($X_{j,t}$) is income before extraordinary items, less preferred dividends, divided by shares outstanding. Consensus analyst forecasts are measured in the month following the earnings announcement. With annual (rather than quarterly) forecasts, econometric concerns relating to stale forecasts are not likely to be material. Forecast dispersion is the standard deviation of consensus analyst forecasts, standardized by consensus analyst forecasts.[8] When the value of this variable for a firm-year is ranked in the *upper* or *lower* half of the sample distribution, we classify this observation as *high* or *low* forecast dispersion, respectively. Finally, we winsorize our variables yearly at the 1 percent level to mitigate possible undue influences of outliers.

In Table 2, we provide descriptive statistics for the full sample and the dispersion subsamples. We find that our regression variables, $P_{j,t}$, $X_{j,t}$, and $f_{j,t}$ are all significantly lower, and $BV_{j,t}$ is significantly higher, when forecast dispersion is high (significance at a 1 percent level). Further, changes in $X_{j,t}$ ($\Delta X_{j,t}$) and market returns ($RET_{j,t}$) are also significantly lower when forecast dispersion is high (significance at a 1 percent level). With the exception of $RET_{j,t}$, these variables are deflated by prior period price, and as a percentage of $P_{j,t-1}$, these differences are as one might expect across the two information environments.

Surprisingly, the mean and median of $v_{j,t}$ are nearly identical across the two subsamples. This may be a result of offsetting positive and negative residuals. Comparing the absolute value of the residuals reveals that the magnitudes of the residuals are significantly greater at a 1 percent level for the high-dispersion firms than the low-dispersion firms. Further, we find that forecast errors are significantly greater, at a 1 percent level, for high-dispersion

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[7] The imbalance between high- and low-dispersion firms results from the odd number of observations per year as well as ties at the 50 percentile being classified as high-dispersion.

[8] Standardizing forecast dispersion by price rather than consensus analyst forecasts does not alter our results or our conclusions.

TABLE 2
Descriptive Statistics

	Full Sample			Low-Dispersion Subsample			High-Dispersion Subsample			Mean Diff. (H–L)
	Mean	Med.	Std. Dev.	Mean	Med.	Std. Dev.	Mean	Med.	Std. Dev.	
$P_{j,t}$	1.183	1.089	0.574	1.252	1.167	0.520	1.113	1.000	0.615	−0.139***
$RET_{j,t}$	0.203	0.112	0.577	0.272	0.187	0.521	0.134	0.021	0.620	−0.138***
$BV_{j,t}$	0.513	0.453	0.304	0.457	0.404	0.264	0.569	0.515	0.330	0.112***
$X_{j,t}$	0.046	0.053	0.063	0.062	0.061	0.041	0.030	0.039	0.076	−0.032***
$\Delta X_{j,t}$	0.007	0.008	0.076	0.013	0.010	0.046	0.002	0.002	0.097	−0.011***
$f_{j,t}$	0.072	0.067	0.040	0.078	0.073	0.035	0.066	0.059	0.043	−0.012***
$\hat{v}_{j,t}$	0.006	0.007	0.029	0.005	0.004	0.022	0.005	0.004	0.032	0.000
$\lvert\hat{v}_{j,t}\rvert$	0.021	0.016	0.020	0.015	0.009	0.016	0.024	0.018	0.022	0.009***
$FE_{j,t}$	0.014	0.004	0.045	0.007	0.001	0.029	0.021	0.009	0.055	0.014***
$s(f_{j,t})$	0.166	0.054	0.936	0.028	0.026	0.017	0.305	0.124	1.308	0.277***
$r_{j,t}$	0.148	0.149	0.295	0.144	0.146	0.278	0.151	0.152	0.312	0.007**
$LG_{j,t}$	0.179	0.150	0.090	0.176	0.150	0.077	0.182	0.150	0.102	0.006***
$MB_{j,t}$	1.346	1.233	0.728	1.259	1.164	0.673	1.434	1.317	0.768	0.175***
$EP_{j,t}$	0.489	0.489	0.509	0.582	0.632	0.522	0.388	0.389	0.474	−0.194***
$FP_{j,t}$	0.157	0.143	0.095	0.150	0.125	0.093	0.165	0.143	0.097	0.015***
$AL_{j,t}$	0.124	0.000	0.329	0.033	0.000	0.178	0.214	0.000	0.410	0.181***

(continued on next page)

TABLE 2 (continued)

$DP_{j,t}$	0.406	0.000	0.491	0.315	0.000	0.464	0.498	0.000	0.500	0.183***
$SZ_{j,t}$	6.295	6.126	1.720	6.345	6.216	1.695	6.245	6.043	1.744	−0.100***

*, **, *** Indicate two-tailed significance at the 10, 5, and 1 percent levels, respectively.

Sample size of full sample is 27,728. Sample size of high-dispersion subsample is 13,875. Sample size of low-dispersion subsample is 13,853.

Forecast dispersion is calculated as the standard deviation of consensus analyst forecasts, standardized by consensus analyst forecasts. When the value of this variable for a firm-year is ranked in the *upper* or *lower* half of the sample distribution, we classify this observation as ***high*** or *low* forecast dispersion, respectively.

Variable Definitions:

$P_{j,t}$ = closing security price on the last trading day of the month following the annual earnings announcement;
$RET_{j,t}$ = compounded daily returns from the last trading day of the month following the annual earnings announcement through the same date the following year;
$BV_{j,t}$ = year-end common equity total, divided by shares outstanding;
$X_{j,t}$ = income before extraordinary items less preferred dividends, divided by shares outstanding;
$\Delta X_{j,t}$ = change in income before extraordinary items less preferred dividends, divided by shares outstanding;
$f_{j,t}$ = consensus analyst forecasts of next year's earnings, measured in the month following the annual earnings announcement;
$\hat{v}_{j,t}$ = consensus analyst forecasts residuals of next year's earnings, calculated as: $f_{j,t} = \delta_0 + \delta_1 X_{j,t} + \delta_2 BV_{j,t} + v_{j,t}$;
$|\hat{v}_{j,t}|$ = absolute value of consensus analyst forecasts residuals of next year's earnings;
$FE_{j,t}$ = consensus analyst forecasts of next year's earnings, measured in the month following the annual earnings announcement less actual I/B/E/S earnings;
$s(f_{j,t})$ = standard deviation of consensus analyst forecasts of next year's earnings, measured in the month following the annual earnings announcement, standardized by $f_{j,t}$;
$r_{j,t}$ = cost of capital (limited to 26,075 observations);
$LG_{j,t}$ = consensus analyst forecasts of long-term growth, measured in the month following the annual earnings announcement;
$MB_{j,t}$ = market beta calculated using the market model with daily returns for year $t-1$ (limited to 26,075 observations);
$EP_{j,t}$ = earnings persistence at the beginning of the year calculated over an eight-year period (limited to 16,329 observations);
$FP_{j,t}$ = 1 divided by the number of forecasts used by I/B/E/S to determine the consensus analyst forecast of next years earnings;
$AL_{j,t}$ = an indicator variable set to 1 if the firm year had a net loss for year t, otherwise 0;
$DP_{j,t}$ = an indicator variable set to 1 if the change in price from the prior year was negative, otherwise 0; and
$SZ_{j,t}$ = the log of inflation adjusted total assets measured at the beginning of year t.

All relevant variables are adjusted for stock splits and stock dividends, and are standardized by the closing security price on the last trading day for the month following the annual earnings announcement of the prior fiscal year.

firms than for low-dispersion firms. These results suggest that analysts base their forecasts on less reliable information when forecast dispersion is high.

We also report descriptive statistics for cost of capital, long-term growth, market beta, earnings persistence, analyst following, accounting losses, previous decreases in market price, and firm size. The prior literature (such as Abarbanell 1991; Collins and Kothari 1989; Teoh and Wong 1993; among others) claims these variables likely affect future earnings and the market pricing of equity. We expect these same variables to be factors that analysts consider when formulating their forecasts. We find that cost of capital, long-term growth, market beta, and analyst precision (inverse of analyst following) are all significantly larger; the occurrence of accounting losses and previous price decreases are significantly higher; and earnings persistence and firm size are significantly smaller for high-dispersion firms (at a 5 percent level, or less).[9]

We present the Pearson correlations in Table 3. As with other research on valuation, we find that $P_{j,t}$ is significantly positively correlated at a 1 percent level with $X_{j,t}$ and $BV_{j,t}$. We find that the correlation between $P_{j,t}$ and $f_{j,t}$ is also significantly positive at a 1 percent level and nearly double the correlation between $P_{j,t}$ and $X_{j,t}$. Additionally, $f_{j,t}$ is significantly positively correlated at a 1 percent level with $X_{j,t}$ and, $BV_{j,t}$ where the correlation indicates the extent that analysts rely on accounting fundamentals to forecast future earnings. Further, $\hat{v}_{j,t}$ and the absolute value of $\hat{v}_{j,t}$ are significantly positively associated at a 1 percent level with $P_{j,t}$, but the correlation is substantially less than the correlation between $f_{j,t}$ and $P_{j,t}$, indicative of the association between analyst forecasts and market prices overlapping with the association of market prices with earnings and book value.

The standard deviation of $f_{j,t}$, $s(f_{j,t})$, serves as our measure of forecast dispersion. As expected, we find $s(f_{j,t})$ is significantly negatively correlated at a 1 percent level with $P_{j,t}$. We also find $s(f_{j,t})$ is significantly negatively correlated at the 1 percent level with $X_{j,t}$, $f_{j,t}$, and $\hat{v}_{j,t}$, and significantly positively correlated at a 1 percent level with the absolute value of $\hat{v}_{j,t}$. Overall, the correlations of our variables with $s(f_{j,t})$ are consistent with how these variables varied across the low and high forecast dispersion subsamples, reported above.

Table 4 presents summary statistics of our first stage estimation of $v_{j,t}$, where $v_{j,t}$ is the residual from regressing analyst forecasts on earnings and book value in Equation (2). We find that the mean of the yearly R^2s is 0.7972. The high R^2s indicate that a substantial proportion of the variations in analyst forecasts are explained by earnings and book values.

When we estimate $v_{j,t}$ by dispersion type, we find that the means of the yearly R^2s are 0.8961 and 0.7345 for the low- and high-dispersion subsamples, respectively. The difference in the means suggests that analysts of high-dispersion firms weight other factors more, and earnings and book value less, in their forecasts than do analysts of low-dispersion firms. This difference is also indicated by the differences in regression coefficients on accounting earnings, with means of 0.669 and 0.300 for low- and high-dispersion firms, respectively.

IV. EMPIRICAL RESULTS

Tests of H1: The Explanatory Power of Analyst Forecasts and of Earnings and Book Value

Table 5 presents results from tests of H1. Panel A reports results from regressing price on earnings, book value, and analyst forecast residuals (EBAF model). We find the mean

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[9] We also perform additional tests of our hypotheses by interacting these variables with the variables in Equations (3) and (4) to identify whether analysts weight these factors more heavily in poor information environments than in good information environments. We report results for these additional tests in Section IV.

TABLE 3
Correlation Matrix

	$P_{j,t}$	$RET_{j,t}$	$BV_{j,t}$	$X_{j,t}$	$\Delta X_{j,t}$	$f_{j,t}$	$\hat{v}_{j,t}$	$\lvert\hat{v}_{j,t}\rvert$	$FE_{j,t}$	$s(f_{j,t})$	$r_{j,t}$	$LG_{j,t}$	$MB_{j,t}$	$EP_{j,t}$	$FP_{j,t}$	$AL_{j,t}$	$DP_{j,t}$
$RET_{j,t}$	0.19 0.001	1.00															
$BV_{j,t}$	0.58 0.001	−0.12 0.001	1.00														
$X_{j,t}$	0.45 0.001	0.03 0.001	0.39 0.001	1.00													
$\Delta X_{j,t}$	0.08 0.001	0.14 0.001	−0.02 0.001	0.51 0.001	1.00												
$f_{j,t}$	0.72 0.001	−0.02 0.001	0.69 0.001	0.65 0.001	0.16 0.001	1.00											
$\hat{v}_{j,t}$	0.30 0.001	0.05 0.001	−0.10 0.001	−0.07 0.001	−0.14 0.001	0.49 0.001	1.00										
$\lvert\hat{v}_{j,t}\rvert$	0.51 0.001	−0.07 0.001	0.49 0.001	0.09 0.001	−0.14 0.001	0.57 0.001	0.43 0.001	1.00									
$FE_{j,t}$	0.11 0.001	−0.16 0.001	0.26 0.001	−0.05 0.001	−0.12 0.001	0.18 0.001	0.10 0.001	0.24 0.001	1.00								
$s(f_{j,t})$	−0.05 0.001	−0.06 0.001	0.01 0.241	−0.13 0.001	−0.07 0.001	−0.09 0.001	−0.05 0.001	0.03 0.001	0.08 0.001	1.00							
$r_{j,t}$	−0.06 0.001	−0.15 0.001	−0.06 0.001	−0.05 0.001	0.00 0.947	−0.05 0.001	0.01 0.268	−0.02 0.004	0.04 0.001	0.00 0.741	1.00						
$LG_{j,t}$	−0.07 0.001	0.15 0.001	−0.28 0.001	−0.23 0.001	0.02 0.001	−0.29 0.001	−0.03 0.001	−0.14 0.001	0.02 0.008	0.01 0.018	0.12 0.001	1.00					
$MB_{j,t}$	−0.13 0.001	0.03 0.001	−0.18 0.001	−0.20 0.001	−0.02 0.001	−0.20 0.001	−0.01 0.068	−0.09 0.001	0.06 0.001	0.04 0.001	0.19 0.001	0.38 0.001	1.00				
$EP_{j,t}$	0.00 0.958	−0.03 0.002	−0.12 0.001	0.06 0.001	−0.02 0.002	−0.02 0.008	0.04 0.001	−0.07 0.001	−0.03 0.001	−0.04 0.001	−0.01 0.230	0.06 0.001	0.02 0.014	1.00			
$FP_{j,t}$	−0.27 0.001	0.00 0.598	−0.12 0.001	−0.14 0.001	−0.03 0.001	−0.19 0.001	−0.09 0.001	−0.10 0.001	0.04 0.001	0.02 0.001	0.01 0.155	0.16 0.001	−0.09 0.001	−0.09 0.001	1.00		

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TABLE 3 (continued)

	$P_{j,t}$	$RET_{j,t}$	$BV_{j,t}$	$X_{j,t}$	$\Delta X_{j,t}$	$f_{j,t}$	$\hat{v}_{j,t}$	$\lvert\hat{v}_{j,t}\rvert$	$FE_{j,t}$	$s(f_{j,t})$	$r_{j,t}$	$LG_{j,t}$	$MB_{j,t}$	$EP_{j,t}$	$FP_{j,t}$	$AL_{j,t}$	$DP_{j,t}$
$AL_{j,t}$	−0.10	−0.10	−0.02	−0.51	−0.32	−0.19	0.12	0.12	0.09	0.20	0.02	0.12	0.17	−0.13	0.07	1.00	
	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.015	0.001	0.001	0.001	0.001		
$DP_{j,t}$	−0.19	−0.63	0.06	−0.09	−0.14	−0.05	−0.06	0.05	0.16	0.08	0.14	0.00	0.06	−0.01	0.06	0.14	1.00
	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.844	0.001	0.112	0.001	0.001	
$SZ_{j,t}$	0.36	−0.08	0.37	0.25	0.00	0.37	0.07	0.26	−0.04	−0.01	−0.08	−0.48	−0.20	−0.05	−0.57	−0.04	−0.03
	0.001	0.001	0.001	0.001	0.669	0.001	0.001	0.001	0.001	0.400	0.001	0.001	0.001	0.001	0.001	0.001	0.001

Each cell presents the Pearson correlation coefficient and its associated p-value.
Sample size of full sample is 27,728.

Variable Definitions:

$P_{j,t}$ = closing security price on the last trading day of the month following the annual earnings announcement;
$RET_{j,t}$ = compounded daily returns from the last trading day of the month following the annual earnings announcement through the same date the following year;
$BV_{j,t}$ = year-end common equity total, divided by shares outstanding;
$X_{j,t}$ = income before extraordinary items less preferred dividends, divided by shares outstanding;
$\Delta X_{j,t}$ = change in income before extraordinary items less preferred dividends, divided by shares outstanding;
$f_{j,t}$ = consensus analyst forecasts of next year's earnings, measured in the month following the annual earnings announcement;
$\hat{v}_{j,t}$ = consensus analyst forecasts residuals of next year's earnings, calculated as: $f_{j,t} = \delta_0 + \delta_1 Z_{j,t} + \delta_2 BV_{j,t} + v_{j,t}$;
$\lvert\hat{v}_{j,t}\rvert$ = absolute value of consensus analyst forecasts residuals of next year's earnings;
$FE_{j,t}$ = consensus analyst forecasts of next year's earnings, measured in the month following the annual earnings announcement less actual I/B/E/S earnings;
$s(f_{j,t})$ = standard deviation of consensus analyst forecasts of next year's earnings, measured in the month following the annual earnings announcement, standardized by $f_{j,t}$;
$r_{j,t}$ = cost of capital (limited to 26,075 observations);
$LG_{j,t}$ = consensus analyst forecasts of long-term growth, measured in the month following the annual earnings announcement;
$MB_{j,t}$ = market beta calculated using the market model with daily returns for year $t-1$ (limited to 26,075 observations);
$EP_{j,t}$ = earnings persistence at the beginning of the year calculated over an eight-year period (limited to 16,329 observations);
$FP_{j,t}$ = 1 divided by the number of forecasts used by I/B/E/S to determine the consensus analyst forecast of next years earnings;
$AL_{j,t}$ = an indicator variable set to 1 if the firm year had a net loss for year t, otherwise 0;
$DP_{j,t}$ = an indicator variable set to 1 if the change in price from the prior year was negative, otherwise 0; and
$SZ_{j,t}$ = the log of inflation adjusted total assets measured at the beginning of year t.

All relevant variables are adjusted for stock splits and stock dividends and are standardized by the closing security price on the last trading day for the month following the annual earnings announcement of the prior fiscal year.

TABLE 4
Summary of Properties from Estimating Forecast Residuals in First-Stage OLS Regression of Analyst Forecasts on Earnings and Book Value

$$f_{j,t} = \delta_0 + \delta_1 X_{j,t} + \delta_2 BV_{j,t} + \nu_{j,t}$$

Coeff.	Exp. Sign	Mean	Std. Err.	Min.	Median	Max
Full Sample						
δ_0	+/−	0.062	0.011	0.037	0.061	0.081
δ_1	+	0.389	0.016	0.372	0.385	0.415
δ_2	+	0.082	0.003	0.078	0.081	0.089
Adj. R^2		0.7972	0.0391	0.7457	0.7910	0.9066
Low (L) Dispersion Subsample						
δ_0	+/−	0.036	0.011	0.008	0.034	0.053
δ_1	+	0.669	0.098	0.554	0.676	0.973
δ_2	+	0.060	0.012	0.023	0.061	0.073
Adj. R^2		0.8961	0.0290	0.8566	0.9012	0.9716
High (H) Dispersion Subsample						
δ_0	+/−	0.072	0.012	0.050	0.070	0.093
δ_1	+	0.300	0.013	0.281	0.297	0.325
δ_2	+	0.079	0.004	0.073	0.078	0.090
Adj. R^2		0.7345	0.0550	0.6610	0.7235	0.8884

Statistics reported are based on the estimates from 20 annual cross-sectional regressions from 1984 to 2003 using the above equation.

Sample size of full sample is 27,728. Sample size of high-dispersion subsample is 13,875. Sample size of low-dispersion subsample is 13,853.

Forecast dispersion is calculated as the standard deviation of consensus analyst forecasts, standardized by consensus analyst forecasts. When the value of this variable for a firm-year is ranked in the *upper* or *lower* half of the sample distribution, we classify this observation as *high* or *low* forecast dispersion, respectively.

Variable Definitions:

$f_{j,t}$ = consensus analyst forecasts of next year's earnings per share, measured in the month following the annual earnings announcement;

$X_{j,t}$ = income before extraordinary items less preferred dividends, divided by shares outstanding;

$BV_{j,t}$ = year-end common equity total, divided by shares outstanding; and

$\hat{\nu}_{j,t}$ = consensus analyst forecasts residuals of next year's earnings per share, calculated using the above equation.

All relevant variables are adjusted for stock splits and stock dividends and are standardized by the closing security price on the last trading day for the month following the annual earnings announcement of the prior fiscal year.

of the adjusted R^2s across the yearly regressions is 0.6769. Consistent with the prior literature, we find that the means of the yearly coefficients on both $X_{j,t}$ and $BV_{j,t}$ are significantly greater than zero at a 1 percent level (means of 3.651 and 1.085, respectively). We also find that the mean of the yearly coefficients on analyst forecast residuals, $\nu_{j,t}$, is 10.392, which is also significant at a 1 percent level.[10]

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[10] We also test our hypotheses by pooling our sample firms across all years. Using White's (1980) adjusted t-statistics and Vuong's (1989) likelihood ratio test, we find similar results to those reported.

TABLE 5

Tests of H1: OLS Regressions of Market Price on (1) Earnings, Book Value, and Forecasts Residuals from a First-Stage Regression; (2) Earnings and Book Value; and (3) Analyst Forecasts

Panel A: (EBAF Model) Results from Regressing Price on Earnings, Book Value, and Forecast Residuals from a First Stage Regression

$$P_{j,t} = \beta_0 + \beta_1 X_{j,t} + \beta_2 BV_{j,t} + \beta_3 \hat{v}_{j,t} + \mu_{j,t}$$

Coeff.	Exp. Sign	Mean	t-statistic
β_0	+/−	2.796	128.33***
β_1	+	3.651	115.94###
β_2	+	1.085	213.53###
β_3	+	10.392	152.49###
R^2_{EBAF}		0.6769	

Panel B: (EB Model) Results from Regressing Price on Earnings and Book Value

$$P_{j,t} = \beta_0 + \beta_1 X_{j,t} + \beta_2 BV_{j,t} + \mu_{j,t}$$

Coeff.	Exp. Sign	Mean	t-statistic
β_0	+/−	3.181	128.48***
β_1	+	3.645	97.95###
β_2	+	1.019	171.28###
R^2_{EB}		0.5519	
$R^2_{EBAF} - R^2_{EB}$		0.1250	5.41###

Panel C: (AF Model) Results from Regressing Price on Analyst Forecasts of Future Earnings

$$P_{j,t} = \pi_0 + \pi_1 f_{j,t} + \kappa_{j,t}$$

Coeff.	Exp. Sign	Mean	t-statistic
π_0	+/−	2.349	110.25***
π_1	+	11.368	334.59###
R^2_{AF}		0.6621	
$R^2_{EBAF} - R^2_{AF}$		0.0148	1.71##

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*, **, *** Indicate two-tailed significance at the 10, 5, and 1 percent levels, respectively.

#, ##, ### Indicate one-tailed significance at the 10, 5, and 1 percent levels, respectively.

Sample size is 27,728.

Reported t-statistics are calculated by estimating a pooled standard deviation over the 20 yearly regressions.

Variable Definitions:

$P_{j,t}$ = closing security price on the last trading day of the month following the annual earnings announcement;
$X_{j,t}$ = income before extraordinary items less preferred dividends, divided by shares outstanding;
$BV_{j,t}$ = year-end common equity total, divided by shares outstanding;
$f_{j,t}$ = consensus analyst forecasts of next year's earnings, measured in the month following the annual earnings announcement; and
$\hat{v}_{j,t}$ = consensus analyst forecasts residuals of next year's earnings, calculated as: $f_{j,t} = \delta_0 + \delta_1 X_{j,t} + \delta_2 BV_{j,t} + v_{j,t}$.

All relevant variables are adjusted for stock splits and stock dividends and are standardized by the closing security price on the last trading day for the month following the annual earnings announcement of the prior fiscal year.

Panel B of Table 5 reports the regression results of the EB model. As with our results reported in Panel A, we find that the means of the yearly coefficients on both $X_{j,t}$ and $BV_{j,t}$

are significantly greater than zero at a 1 percent level (means of 3.645 and 1.019, respectively). The mean of the yearly adjusted R^2s, however, is only 0.5519. The 0.1250 difference between R^2_{EBAF} and R^2_{EB} is significant at the 1 percent level, supporting H1a, thus providing evidence that analyst forecasts exhibit significant explanatory power beyond that of earnings and book value.

Hypothesis 1b tests whether the market prices analyst forecasts as fully subsuming the information reflected by the accounting fundamentals. To address this question, we compare the adjusted R^2 of the EBAF model to that of the AF model. Panel C of Table 5 reports the results from regressing the AF model. We find that the mean of the yearly coefficients on $f_{j,t}$ is 11.368, which is significant at a 1 percent level, and the mean of the yearly adjusted R^2s is 0.6621.

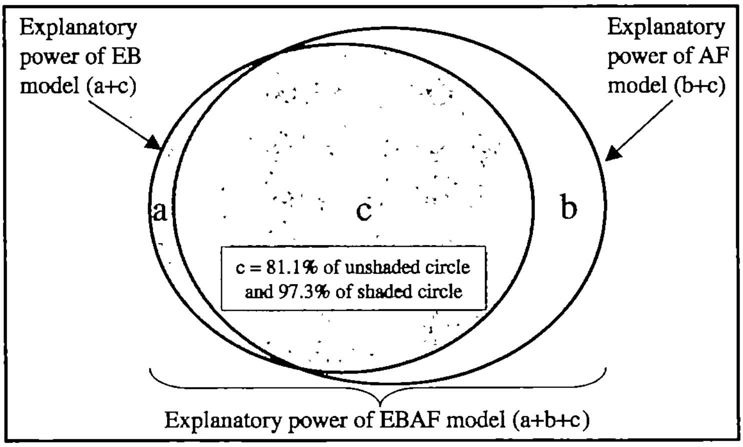
The difference between R^2_{EBAF} and R^2_{AF} is only 0.0148 (significant at the 5 percent level). While statistically supporting H1b, the magnitude of this result suggests that Ohlson's (2001) proposed model, on average, does not economically improve upon a naïve model.

To illustrate the implications of our findings, we graph in Figure 1 the explanatory power of the EBAF, EB, and AF models reported in Table 5, using a Venn diagram. The area of the box represents the set of information that explains market prices and the two circles jointly represent the explanatory power of the EBAF model (R^2_{EBAF}), which comprises

FIGURE 1

Explanatory Power of Regressing Price on: (1) Earnings, Book Value, and Analyst Forecast Residuals, Estimated by a First-Stage Regression (EBAF model); (2) Earnings and Book Value (EB model); and (3) Analyst Forecasts (AF model)

Full Sample


Explanatory power of EB model (a+c)
Explanatory power of AF model (b+c)
a
c
b
c = 81.1% of unshaded circle and 97.3% of shaded circle
Explanatory power of EBAF model (a+b+c)

a = 1.48% b = 12.50% c =53.71%

The values reported are based on the adjusted R^2s reported on Table 5.

"a" is the area of the shaded circle that does not intersect the unshaded circle, representing the explanatory power of earnings and book value beyond that of analyst forecasts.

"b" is the area of the unshaded circle that does not intersect the shaded circle, representing the explanatory power of analyst forecasts beyond that of the accounting fundamentals.

"c" is the area of the shaded circle that intersects with the unshaded circle, representing the explanatory power of the information analyst forecasts capture that is reflected by earnings and book value.

67.69 percent of the box's area. Individually, however, the shaded circle that represents the explanatory power of the EB model (R^2_{EB}) covers 55.19 percent of the box; and, the unshaded circle that represents the explanatory power of the AF model (R^2_{AF}), covers 66.21 percent of the box's area.

The area of the shaded circle that does not intersect the unshaded circle (labeled "a") is the difference between R^2_{EBAF} and R^2_{AF} (1.48 percent) and represents the explanatory power of earnings and book value beyond that of analyst forecasts. The area of the unshaded circle that does not intersect the shaded circle (labeled "b") is the difference between R^2_{EBAF} and R^2_{EB} (12.50 percent), and represents the explanatory power of analyst forecasts beyond that of the accounting fundamentals. The size of the area covered by "a" and "b," therefore, represents the extent of support for H1.

Additionally, the intersection of the two circles (labeled "c") represents the explanatory power of the information analyst forecasts capture that is reflected by earnings and book value, and covers approximately 53.71 percent of the box. Area "c," however, also covers 81.1 percent of the unshaded circle, representing the extent that analyst forecasts rely on the value-relevant information in earnings and book value; and, 97.3 percent of the shaded circle, representing the extent that information reflected by earnings and book value is captured by analyst forecasts. In summary, we find that, on average, analysts forecasts incorporate almost all of the value-relevant information reflected by earnings and book value, and this information comprises a substantial portion of the value-relevant information captured by analyst forecasts.

Tests of H2: Differences across High and Low Forecast Dispersion

Table 6 presents results from tests of H2. Panel A provides the results by dispersion type for the EBAF model. We find that the means of the yearly adjusted R²s are 0.7051 and 0.6360 for the low- and high-dispersion subsamples, respectively (difference of 0.0691 is significant at a 1 percent level). Further, we find that the means of the yearly coefficients on $X_{j,t}$, $BV_{j,t}$, and $\hat{v}_{j,t}$ are positive and significant (at a 1 percent level) for both subsamples, but the means for $X_{j,t}$ and $\hat{v}_{j,t}$ are both significantly lower (at a 1 percent level) for the high-dispersion subsample, as Ohlson's (1995, 2001) model predicts.[11]

Panels B and C of Table 6 report the results from regressing the EB and AF models by dispersion type, respectively. For the EB model, we find that the means of the yearly adjusted R²s are 0.6109 and 0.5224, respectively for the low- and high-dispersion subsamples (difference of 0.0885 is significant at a 5 percent level). We also find similar results for the means of the yearly coefficients on $X_{j,t}$ and $BV_{j,t}$ across the subsamples as those reported for the EBAF model. For the AF model, we find that the means of the yearly adjusted R²s are 0.7025 and 0.5947, respectively for the low- and high-dispersion subsamples (difference of 0.1078 is significant at a 5 percent level). Further, the means of the yearly coefficients on $f_{j,t}$ are 11.991 and 10.451 (both significant at a 1 percent level) respectively for the low- and high-dispersion subsamples, where the difference is significant at a 1 percent level, as expected.

In Figure 2, we compare the adjusted R²s reported on Table 6 to illustrate how the explanatory power of analyst forecasts and of earnings and book value vary by dispersion type. Panel A presents our findings for the low-dispersion subsample and Panel B presents our findings for the high-dispersion subsample.

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[11] Expectations on how the coefficients on book value will vary across dispersion type are contradictory, therefore we offer no prediction for $BV_{j,t}$. The differences in the means of the yearly coefficients on $BV_{j,t}$ we report, however, are consistent with the abandonment hypothesis suggested by Barth et al. (1998).

TABLE 6
Tests of H2: OLS Regressions of Market Price on (1) Earnings, Book Value, and Forecasts Residuals from a First-Stage Regression; (2) Earnings and Book Value; and, (3) Analyst Forecasts—Partitioned by Dispersion Level

Coeff.	Exp. Sign	Mean	t-statistic
Panel A: (EBAF Model) Results from Regressing Price on Earnings, Book Value, and Forecast Residuals from a First Stage Regression			
$P_{j,t} = \beta_0 + \beta_1 X_{j,t} + \beta_2 BV_{j,t} + \beta_3 \hat{\upsilon}_{j,t} + \mu_{j,t}$			
Low (L) Dispersion Subsample			
β_0	+/−	2.886	78.39***
β_1	+/−	7.681	103.11###
β_2	+	0.776	71.35###
β_3	+	12.103	93.96###
R^2_{EBAF}		0.7051	
High (H) Dispersion Subsample			
β_0	+/−	2.644	92.18***
β_1	+	2.122	55.79###
β_2	+	1.061	169.62###
β_3	+	8.432	96.83###
R^2_{EBAF}		0.6360	
Test of Differences (H−L) between High- and Low-Dispersion Subsamples			
$\Delta\beta_0$	+/−	−0.242	−5.01***
$\Delta\beta_1$	−	−5.559	−65.24###
$\Delta\beta_2$	+/−	0.285	22.36***
$\Delta\beta_3$	−	−3.671	−23.31###
ΔR^2_{EBAF}		−0.0691	−1.55
Panel B: (EB Model) Results from Regressing Price on Earnings and Book Value			
$P_{j,t} = \beta_0 + \beta_1 X_{j,t} + \beta_2 BV_{j,t} + \mu_{j,t}$			
Low (L) Dispersion Subsample			
β_0	+/−	3.411	82.73***
β_1	+	7.478	88.44###
β_2	+	0.730	59.23###
R^2_{EB}		0.6109	
$R^2_{EBAF} - R^2_{EB}$		0.0942	2.36##
High (H) Dispersion Subsample			
β_0	+/−	2.925	91.32***
β_1	+	2.081	47.71###
β_2	+	1.003	140.82###
R^2_{EB}		0.5224	
$R^2_{EBAF} - R^2_{EB}$		0.1136	3.97###

(continued on next page)

TABLE 6 (continued)

Coeff.	Exp. Sign	Mean	t-statistic
Test of Differences (H−L) between High- and Low-Dispersion Subsamples			
$\Delta\beta_0$	+/−	−0.486	−12.32***
$\Delta\beta_1$	−	−5.397	−150.69###
$\Delta\beta_2$	+/−	0.273	47.24***
ΔR^2_{EB}		−0.0885	−1.76##
$\Delta(R^2_{EBAF} - R^2_{EB})$		0.0194	1.73##
Panel C: (AF Model) Results from Regressing Price on Analyst Forecasts of Future Earnings			
$P_{j,t} = \pi_0 + \pi_1 f_{j,t} + \kappa_{j,t}$			
Low (L) Dispersion Subsample			
π_0	+/−	2.468	67.10***
π_1	+	11.991	258.97###
R^2_{AF}		0.7025	
$R^2_{EBAF} - R^2_{AF}$		0.0026	0.50
High (H) Dispersion Subsample			
π_0	+/−	2.466	87.47***
π_1	+	10.451	203.47###
R^2_{AF}		0.5947	
$R^2_{EBAF} - R^2_{AF}$		0.0413	2.13##
Test of Differences (H−L) between High- and Low-Dispersion Subsamples			
$\Delta\pi_0$	+/−	−0.002	−0.05
$\Delta\pi_1$	−	−1.540	−22.17###
ΔR^2_{AF}		−0.1078	−2.49##
$\Delta(R^2_{EBAF} - R^2_{AF})$		0.0387	8.41###

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*, **, *** Indicate two-tailed significance at the 10, 5, and 1, percent levels, respectively.
#, ##, ### Indicate one-tailed significance at the 10, 5, and 1, percent levels, respectively.
Sample size of high-dispersion subsample is 13,875. Sample size of low-dispersion subsample is 13,853.
Forecast dispersion is calculated as the standard deviation of consensus analyst forecasts, standardized by consensus analyst forecasts. When the value of this variable for a firm-year is ranked in the *upper* or *lower* half of the sample distribution, we classify this observation as *high* or *low* forecast dispersion, respectively.
Reported t-statistics are calculated by estimating a pooled standard deviation over the 20 yearly regressions.
The variables are defined as follows:

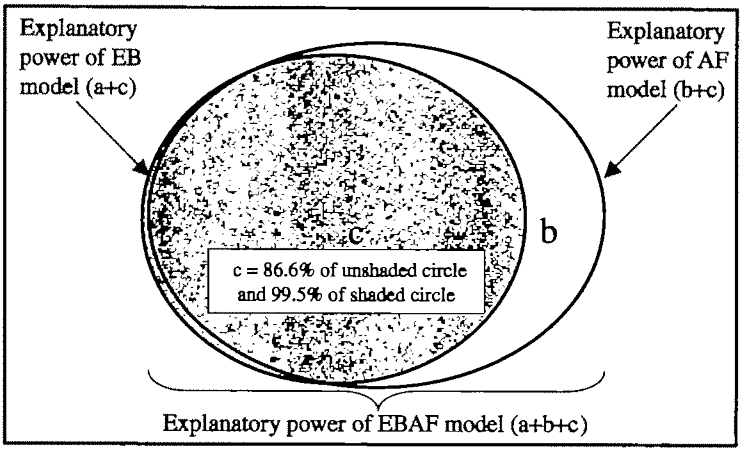
$P_{j,t}$ = closing security price on the last trading day of the month following the annual earnings announcement;
$X_{j,t}$ = income before extraordinary items less preferred dividends, divided by shares outstanding;
$BV_{j,t}$ = year-end common equity total, divided by shares outstanding;
$f_{j,t}$ = consensus analyst forecasts of next year's earnings, measured in the month following the annual earnings announcement; and
$\hat{v}_{j,t}$ = consensus analyst forecasts residuals of next year's earnings, calculated as: $f_{j,t} = \delta_0 + \delta_1 X_{j,t} + \delta_2 BV_{j,t} + v_{j,t}$.

All relevant variables are adjusted for stock splits and stock dividends and are standardized by the closing security price on the last trading day for the month following the annual earnings announcement of the prior fiscal year.

FIGURE 2

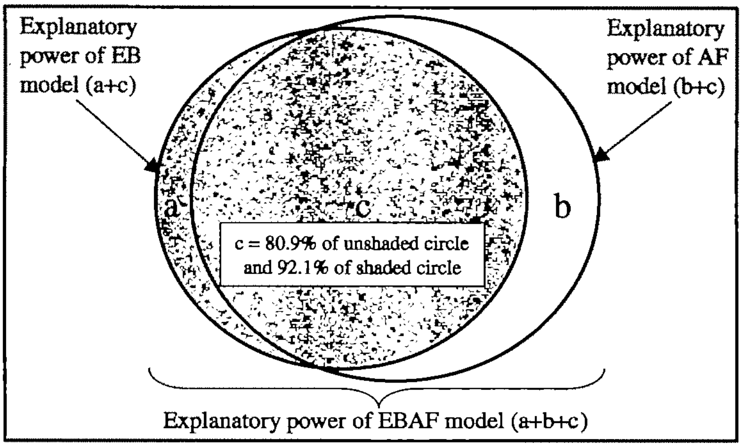
Explanatory Power of Regressing Price on: (1) Earnings, Book Value, and Analyst Forecast Residuals, Estimated by a First-Stage Regression (EBAF model); (2) Earnings and Book Value (EB model); and (3) Analyst Forecasts (AF model)
By Dispersion Type

Panel A: Low-Dispersion Subsample


Explanatory power of EB model (a+c)
Explanatory power of AF model (b+c)
c
b
c = 86.6% of unshaded circle and 99.5% of shaded circle
Explanatory power of EBAF model (a+b+c)

a = 0.26% b = 9.42% c = 60.83%

Panel B: High-Dispersion Subsample


Explanatory power of EB model (a+c)
Explanatory power of AF model (b+c)
a
c
b
c = 80.9% of unshaded circle and 92.1% of shaded circle
Explanatory power of EBAF model (a+b+c)

a = 4.13% b = 11.36% c = 48.11%

The values reported are based on the adjusted R^2s reported on Table 6.
"a" is the area of the shaded circle that does not intersect the unshaded circle, representing the explanatory power of earnings and book value beyond that of analyst forecasts
"b" is the area of the unshaded circle that does not intersect the shaded circle, representing the explanatory power of analyst forecasts beyond that of the accounting fundamentals.
"c" is the area of the shaded circle that intersects with the unshaded circle, representing the explanatory power of the information analyst forecasts capture that is reflected by earnings and book value.
Forecast dispersion is the standard deviation of consensus analyst forecasts, standardized by consensus analyst forecasts. When the value of this variable for a firm-year is ranked in the *upper* or *lower* half of the sample distribution, we classify this observation as *high* or *low* forecast dispersion, respectively.

As with the previous figure, area "a" represents the explanatory power of earnings and book value that is uncorrelated with the explanatory power of analyst forecasts. As shown in Panel C of Table 6, $R^2_{EBAF} - R^2_{AF}$ for the low-dispersion subsample is not significantly different from zero, therefore area "a" is almost nonexistent for the low-dispersion subsample. Area "b" again represents the explanatory power of analyst forecasts that is uncorrelated with the explanatory power of earnings and book value, and covers 9.42 percent ($R^2_{EBAF} - R^2_{EB}$) of the box (significant at a 5 percent level) for the low-dispersion subsample.

In contrast, area "a" for the high-dispersion subsample is 4.13 percent (significant at a 1 percent level), thus providing evidence that when forecast dispersion is high, the explanatory power of earnings and book value is not fully subsumed by the explanatory power of analyst forecasts. Area "b" for the high-dispersion subsample is 11.36 percent (significant at a 1 percent level), again providing evidence that a significant portion of the explanatory power of analyst forecasts is uncorrelated with the explanatory power of earnings and book value.

Hypothesis 2 is supported if areas "a" and "b" in Panel B of Figure 2 are significantly greater than those areas in Panel A. These differences in areas "a" and "b" represent how the incremental explanatory power of earnings and book value and of analyst forecasts, or $\Delta(R^2_{EBAF} - R^2_{AF})$ and $\Delta(R^2_{EBAF} - R^2_{EB})$, respectively, vary across dispersion type. From Panel B of Table 6, we find $\Delta(R^2_{EBAF} - R^2_{EB})$ is 0.0194 (significant at a 5 percent level). From Panel C of Table 6, we find $\Delta(R^2_{EBAF} - R^2_{AF})$ is 0.0387 (significant at a 1 percent level). Together, these results support both H2a and H2b and indicate that analysts shift their reliance from earnings and book value to other value-relevant factors in poor information environments.

In comparing area "c" across the two panels of Figure 2, we find that analysts capture 99.5 percent of the information in earnings and book value, and this information comprises 86.6 percent of the explanatory power of analyst forecasts, when forecast dispersion is low. When forecast dispersion is high, analysts capture 92.1 percent of the information in earnings and book value, but this information comprises only 80.9 percent of the explanatory power of analyst forecasts. Together, these results suggest that Ohlson's (2001) proposed model is most descriptive in poor information environments, where analysts are forced to base a substantial portion of their forecasts on other factors beyond those reflected by earnings and book value.

Additional Sensitivity Tests

Returns Regression Results

The findings reported above are based on price-level regressions. Easton (1999) argues, however, that by first-differencing the price-level regressions, returns regressions mitigate the omitted variables problem and problems relating to nonstationarity in prices. We, therefore, retest our hypothesis using returns regressions. The returns regressions are based on first-differences of Equation (6). Our estimation of "other information" in the returns format becomes changes in other information ($\Delta v_{j,t}$), which is estimated by first-differencing Equation (5).

From untabulated results, we find that $\Delta\hat{v}_{j,t}$ and $\Delta f_{j,t}$ are both significantly greater than zero at the 1 percent level. Also, the explanatory power of the EBAF model is significantly greater than that of the EB model, and to a lesser extent greater than that of the AF model, consistent with our price-level regression results.

In comparing low- and high-dispersion firms, we find the coefficients on $\Delta\hat{v}_{j,t}$ and $\Delta f_{j,t}$ to be lower for high-dispersion firms and the difference between R^2_{EBAF} and R^2_{EB}, and between

R^2_{EBAF} and R^2_{AF} are significantly larger at the 1 percent level for the high-dispersion than low-dispersion firms, again consistent with our price-level regression results.

Alternative Partition Variables and Perfect Forecasts

Thus far, our partitions use only forecast dispersion to represent the information environment. The literature also claims forecast error, analyst following and firm size are highly correlated with information asymmetry (for example, see Brown et al. 1987), and would also likely be effective proxies for the information environment. We therefore repeat our tests by partitioning on each of these variables. We find (from untabulated results) that the differences in adjusted R^{2}s persist, but to a lesser extent, across low and high forecast errors and low and high analyst followings, but not (at conventional levels) across small and large firms.

Barron et al. (1998) point out that forecast dispersion is closely tied to both measurement error and information asymmetry. To strengthen the link between our empirical proxy and information asymmetry, we repeat our tests by replacing analyst forecasts with perfect forecasts, as proposed in Tse and Yaansah (1999). In their study, they use realized future earnings to proxy for perfect forecasts. When we regress realized future earnings in our regressions, we find the coefficients on $\hat{v}_{j,t}$ are dampened. The differences we document between R^2_{EBAF} and R^2_{EB} and between R^2_{EBAF} and R^2_{AF}, however, persist. Our use of realized future earnings to proxy for perfect forecasts mitigates, to some extent, the measurement error inherent in using analyst forecasts as a proxy for "other information." Earnings realizations, however, are substantially different from expectations and likely inject nearly as much noise into our metric as they control, therefore care should be exercised when interpreting the perfect forecast results.

Inclusion of Controls for Industry Effects and Other Variables

Prior studies have documented systematic industry effects in valuation. We control for industry effects by including control variables for the three largest industries represented by our sample, specifically, we control for durable goods, retailers, and other manufacturers. We find (from untabulated results) that the regression coefficients on $X_{j,t}$ and $\hat{v}_{j,t}$, or on $f_{j,t}$, are higher for those firms in the retail and other manufacturer industries, and lower for those firms in the durable goods industry. The differences in adjusted R^{2}s, however, persist after including these control variables. We also partition earnings into operating cash flows and accruals. We anticipate that by allowing separate coefficients on the components of earnings, the weighting on analyst forecasts will decrease, particularly for the high-dispersion subsample. The (untabulated) results indicate, however, no significant differences from the results reported.

Finally, since our results suggest that analysts rely more heavily on other factors than on accounting earnings and book value when forecast dispersion is high, we retest our hypotheses by including the additional variables presented in Table 2 to identify which factors analysts consider in poor information environments. Specifically, we include measures of long-term growth, market beta, earnings persistence, analyst forecast precision, accounting losses, previous decreases in price, and firm size. Since our focus is to identify how analyst forecasts vary across dispersion type, we retest the EBAF and AF models by including a dummy (0,1) indicator variable, H, that represents those firms from the high-dispersion subsample. We include H as its own regressor to control for systematic differences across dispersion type and interact H with each regressor. The results from retesting the EBAF and AF models are reported in Table 7.

TABLE 7
Additional Tests: OLS Regressions of Market Price on (1) Earnings, Book Value, and Forecasts Residuals from a First-Stage Regression; and (2) Analyst Forecasts—Interacted with Other Factors and a High-Dispersion Indicator Variable

Panel A: (EBAF Model) Results from Regressing Price on Earnings, Book Value, and Forecast Residuals from a First-Stage Regression

$$P_{j,t} = \beta_0 + \beta_1 X_{j,t} + \beta_2 BV_{j,t} + \beta_3 \hat{\nu}_{j,t} + \sum_i OF_i(\beta_{0,i} + \beta_{1,i} X_{j,t} + \beta_{2,i} BV_{j,t} + \beta_{3,i} \hat{\nu}_{j,t})$$
$$+ \beta_{0,H} H + \beta_{1,H} H{*}X_{j,t} + \beta_{2,H} H{*}BV_{j,t} + \beta_{3,H} H{*}\hat{\nu}_{j,t} + \sum_i H{*}OF_i{*}(\beta_{0,H,i} + \beta_{1,H,i} X_{j,t}$$
$$+ \beta_{2,H,i} BV_{j,t} + \beta_{3,H,i} \hat{\nu}_{j,t}) + \mu_{j,t}$$

Coeff.	**Exp. Sign**	**Mean**	**t-statistic**
β_0	+/−	0.035	0.07
$\beta_{0,LG}$	+/−	0.044	8.95***
$\beta_{0,MB}$	+/−	2.690	42.18***
$\beta_{0,EP}$	+/−	0.694	6.92***
$\beta_{0,FP}$	+/−	−6.147	−16.71***
$\beta_{0,AL}$	+/−	−0.576	−1.05
$\beta_{0,DP}$	+/−	−1.571	−7.62***
$\beta_{0,SZ}$	+/−	0.226	9.42***
β_1	+	16.011	16.80###
$\beta_{1,LG}$	+/−	0.150	13.51***
$\beta_{1,MB}$	+/−	−1.637	−12.81***
$\beta_{1,EP}$	+/−	2.390	13.25***
$\beta_{1,FP}$	+/−	−10.493	−14.42***
$\beta_{1,AL}$	+/−	−5.206	−6.94***
$\beta_{1,DP}$	+/−	−1.242	−6.45***
$\beta_{1,SZ}$	+/−	0.132	3.25***
β_2	+	−0.399	−2.99###
$\beta_{2,LG}$	+/−	0.021	12.69***
$\beta_{2,MB}$	+/−	−0.119	−6.20***
$\beta_{2,EP}$	+/−	−0.243	−8.89***
$\beta_{2,FP}$	+/−	0.086	0.80
$\beta_{2,AL}$	+/−	0.107	0.51
$\beta_{2,DP}$	+/−	−0.100	−3.08***
$\beta_{2,SZ}$	+/−	−0.025	−4.11***
β_3	+	6.966	4.33###
$\beta_{3,LG}$	+/−	0.223	12.79***
$\beta_{3,MB}$	+/−	−1.918	−8.62***
$\beta_{3,EP}$	+/−	−0.247	−0.80
$\beta_{3,FP}$	+/−	−8.999	−6.99***
$\beta_{3,AL}$	+/−	−2.338	−1.96**
$\beta_{3,DP}$	+/−	−3.013	−8.68***
$\beta_{3,SZ}$	+/−	−0.449	−6.59***
$\beta_{0,H}$	+/−	−0.077	−0.12
$\beta_{0,H,LG}$	+/−	0.003	0.51

(*continued on next page*)

TABLE 7 (continued)

Coeff.	Exp. Sign	Mean	t-statistic
$\beta_{0,H,MB}$	+/−	−2.513	−32.16***
$\beta_{0,H,EP}$	+/−	−0.230	−1.61
$\beta_{0,H,FP}$	+/−	2.058	4.21***
$\beta_{0,H,AL}$	+/−	0.053	0.09
$\beta_{0,H,DP}$	+/−	0.122	0.46
$\beta_{0,H,SZ}$	+/−	−0.232	−7.41***
$\beta_{1,H}$	+/−	−13.804	−12.93***
$\beta_{1,H,LG}$	+/−	−0.092	−7.46***
$\beta_{1,H,MB}$	+/−	1.343	9.30***
$\beta_{1,H,EP}$	+/−	−2.338	−10.73***
$\beta_{1,H,FP}$	+/−	12.802	15.25***
$\beta_{1,H,AL}$	+/−	4.360	5.38***
$\beta_{1,H,DP}$	+/−	1.170	5.31***
$\beta_{1,H,SZ}$	+/−	−0.272	−5.74***
$\beta_{2,H}$	+/−	1.841	12.19***
$\beta_{2,H,LG}$	+/−	−0.011	−6.21***
$\beta_{2,H,MB}$	+/−	0.268	12.07***
$\beta_{2,H,EP}$	+/−	0.212	6.24***
$\beta_{2,H,FP}$	+/−	−0.996	−7.78***
$\beta_{2,H,AL}$	+/−	−0.015	−0.07
$\beta_{2,H,DP}$	+/−	−0.137	−3.44***
$\beta_{2,H,SZ}$	+/−	0.046	6.30***
$\beta_{3,H}$	+/−	3.569	1.85*
$\beta_{3,H,LG}$	+/−	−0.095	−4.67***
$\beta_{3,H,MB}$	+/−	1.868	7.08***
$\beta_{3,H,EP}$	+/−	1.030	2.56***
$\beta_{3,H,FP}$	+/−	5.351	3.37***
$\beta_{3,H,AL}$	+/−	2.342	1.84*
$\beta_{3,H,DP}$	+/−	0.349	0.78
$\beta_{3,H,SZ}$	+/−	−0.214	−2.51**

Panel B: (AF Model) Results from Regressing Price on Analyst Forecasts of Future Earnings

$$P_{j,t} = \pi_0 + \pi_1 f_{j,t} + \sum_i OF_i{*}(\pi_{0,i} + \pi_{1,i} f_{j,t}) + \pi_{0,H} H \\ + \pi_{1,H} H{*} f_{j,t} + \sum_i H{*}OF_i{*}(\pi_{0,H,i} + \pi_{1,H,i} f_{j,t}) + \kappa_{j,t}$$

Coeff.	Exp. Sign	Mean	t-statistic
π_0	+/−	0.991	2.11**
$\pi_{0,LG}$	+/−	0.034	7.67***
$\pi_{0,MB}$	+/−	0.492	7.85***
$\pi_{0,EP}$	+/−	0.757	7.64***
$\pi_{0,FP}$	+/−	−5.424	−14.89***
$\pi_{0,AL}$	+/−	0.457	1.17
$\pi_{0,DP}$	+/−	−1.380	−8.03***
$\pi_{0,SZ}$	+/−	0.221	9.13***

(*continued on next page*)

TABLE 7 (continued)

Coeff.	**Exp. Sign**	**Mean**	**t-statistic**
π_1	+	10.792	20.92###
$\pi_{1,LG}$	+/−	0.244	36.22***
$\pi_{1,MB}$	+/−	0.208	2.54**
$\pi_{1,EP}$	+/−	0.671	6.39***
$\pi_{1,FP}$	+/−	−9.259	−19.80***
$\pi_{1,AL}$	+/−	−0.527	−1.05
$\pi_{1,DP}$	+/−	−1.912	−10.74***
$\pi_{1,SZ}$	+/−	−0.065	−2.36**
$\pi_{0,H}$	+/−	−0.022	−0.04
$\pi_{0,H,LG}$	+/−	−0.010	−1.78*
$\pi_{0,H,MB}$	+/−	−2.680	−34.23***
$\pi_{0,H,EP}$	+/−	−0.452	−3.23***
$\pi_{0,H,FP}$	+/−	3.857	7.93***
$\pi_{0,H,AL}$	+/−	−0.297	−0.69
$\pi_{0,H,DP}$	+/−	0.336	1.46
$\pi_{0,H,SZ}$	+/−	−0.043	−1.35
$\pi_{1,H}$	+/−	1.751	2.29**
$\pi_{1,H,LG}$	+/−	−0.148	−15.88***
$\pi_{1,H,MB}$	+/−	3.294	28.47***
$\pi_{1,H,EP}$	+/−	0.752	4.32***
$\pi_{1,H,FP}$	+/−	4.452	6.31***
$\pi_{1,H,AL}$	+/−	1.341	2.40**
$\pi_{1,H,DP}$	+/−	0.209	0.79
$\pi_{1,H,SZ}$	+/−	−0.024	−0.58

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*, **, *** Indicate two-tailed significance at the 10, 5, and 1, percent levels, respectively.
#, ##, ### Indicate one-tailed significance at the 10, 5, and 1, percent levels, respectively.

Sample size of high-dispersion subsample is 13,875. Sample size of low-dispersion subsample is 13,853. Total sample size is reduced by 1,653 observations for the market beta tests and by 11,399 for the earnings persistence tests.

Forecast dispersion is calculated as the standard deviation of consensus analyst forecasts, standardized by consensus analyst forecasts. When the value of this variable for a firm-year is ranked in the *upper* or *lower* half of the sample distribution, we classify this observation as *high* or *low* forecast dispersion, respectively.

Reported t-statistics are calculated by estimating a pooled standard deviation over the 20 yearly regressions.

The variables are defined as follows:

$P_{j,t}$ = closing security price on the last trading day of the month following the annual earnings announcement;
$X_{j,t}$ = income before extraordinary items less preferred dividends, divided by shares outstanding;
$BV_{j,t}$ = year-end common equity total, divided by shares outstanding;
$f_{j,t}$ = consensus analyst forecasts of next year's earnings, measured in the month following the annual earnings announcement; and
$\hat{v}_{j,t}$ = consensus analyst forecasts residuals of next year's earnings, calculated as: $f_{j,t} = \delta_0 + \delta_1 X_{j,t} + \delta_2 BV_{j,t} + v_{j,t}$.

The other factor indicator (OF_t) is set to $LG_{j,t}$, $MB_{j,t}$, $EP_{j,t}$, $FP_{j,t}$, $AL_{j,t}$, $DP_{j,t}$ and, $SZ_{j,t}$, where:

$LG_{j,t}$ = consensus analyst forecasts of long-term growth, measured in the month following the annual earnings announcement;
$MB_{j,t}$ = market beta calculated using the market model with daily returns for year $t-1$ (limited to 26,075 observations);
$EP_{j,t}$ = earnings persistence at the beginning of the year calculated over an eight-year period (limited to 16,329 observations);
$FP_{j,t}$ = 1 divided by the number of forecasts used by I/B/E/S to determine the consensus analyst forecast of next years earnings;

(*continued on next page*)

TABLE 7 (continued)

$AL_{j,t}$ = an indicator variable set to 1 if the firm year had a net loss for year t, otherwise 0;
$DP_{j,t}$ = an indicator variable set to 1 if the change in price from the prior year was negative, otherwise 0; and
$SZ_{j,t}$ = the log of inflation adjusted total assets measured at the beginning of year t.

All relevant variables are adjusted for stock splits and stock dividends and are standardized by the closing security price on the last trading day for the month following the annual earnings announcement of the prior fiscal year.

Our results indicate that analysts consistently significantly weight (at a 1 percent level) long-term growth less, and market beta, earnings persistence, and forecast precision more, when forecast dispersion is high. Accounting losses are weighted more in the AF model at a 5 percent level of significance, but at a 10 percent level in the EBAF model. We find, however, that previous price decreases and firm size are not consistently considered by analysts. Overall, Table 7 provides evidence of other factors not reflected by earnings and book value that analysts incorporate into their forecasts when the information environment is poor.

V. CONCLUSION

This study provides evidence in support of Ohlson's (2001) proposition that consensus analyst forecasts are an effective proxy for other information in a model that regresses market prices on earnings, book value, and other information. Similar to Dechow et al. (1999), we find that the inclusion of analyst forecasts significantly improves the explanatory power of the model. The explanatory power of earnings and book value relative to that of analyst forecasts, however, is on average very small. This cross-sectional evidence suggests analysts are, on average, fully incorporating the information reflected in earnings and book value, plus significantly more information beyond earnings and book value.

In poor information environments, we find that the explanatory power of analysts and of earnings and book value are significantly lower than in good information environments, consistent with the predictions of Ohlson's (1995, 2001) valuation model. In poor information environments, however, our findings suggest that poor earnings quality forces analysts to weight more heavily other value-relevant factors in their forecasts, factors not reflected in earnings and book value. We find that this shift by analysts away from relying on accounting fundamentals results in the explanatory power of analyst forecasts overlapping to a lesser extent with the explanatory power of earnings and book value. Likewise, we find that this shift results in the explanatory power of earnings and book value overlapping to a lesser extent with the explanatory power of analyst forecasts. Our study, we believe, is the first to calibrate the extent to which these shifts occur.

This study is likely to benefit practitioners in their efforts to determine the appropriate weights to place on earnings, book value, and other information when valuing a firm. Further, we demonstrate to practitioners the importance of shifting weights when the information environment deteriorates. For accounting researchers, this study provides guidance on how to improve the power of the valuation models used in future research. While it may be intuitive to believe that the explanatory power of analyst forecasts or of earnings and book value would differ across information environments, studies to date have failed to exercise such controls.

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THE ACCOUNTING REVIEW
Vol. 82, No. 3
2007
pp. 679–704

Strategic Errors and Audit Quality: An Experimental Investigation

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ABSTRACT: We report experimental results for a simple bimatrix game between a manager and an auditor. We investigate whether the quality of audited financial statements improves if the relative proportion of perfectly honest auditors increases. We find that the quality of audited reports is higher if computerized auditors who always perform a high audit effort are present. This result is in direct contrast to the Nash prediction for our game. The Quantal Response Equilibrium, which explicitly takes into account boundedly rational behavior, supports our experimental data. Our main findings depend neither on framing nor the payoff matrix we use.

Keywords: *auditing; experimental economics; game theory; bounded rationality.*

Data Availability: *Data and instructions are available from the authors on request.*

I. INTRODUCTION

The recent wave of accounting scandals has opened a debate among academics, regulators, and the accounting profession on how to restore public faith in corporate financial disclosures. The discussion focuses on managers who have taken advantage of overly "aggressive" accounting, and who use misleading, possibly even fraudulent, accounting practices. In addition, the auditing profession has been fiercely criticized for its insufficient effort, both when performing an audit and when reporting the findings to the public (see [Zeff 2003a, 2003b] for an overview).

Simple bimatrix games have been presented in the auditing literature to analyze the strategic interdependence between the manager's reporting strategy and the auditor's effort choice (see, among others, Magee 1980; Fellingham and Newman 1985). In these models,

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We appreciate the helpful comments of Robert J. Bloomfield, Armin Falk, Ernst Fehr, Jayanthi Krishnan, Peter Ockenfels, Jeroen Suijis, and participants in the E.I.A.S.M. Workshop on Accounting and Economics V in Madrid, Spain, the European Economic Science Association Meeting in Strasbourg, France, the 14th Asian-Pacific Conference on International Accounting Issues in Los Angeles, U.S., the Annual Congress of the European Accounting Association in Seville, Spain, and the Annual Meeting of the American Accounting Association in Honolulu, Hawaii. Financial support from the Swiss National Science Foundation (project no. 12-67751.02), the Ludwig Boltzmann Institute for the Analysis of Economic Growth, the Network on the Evolution of Preferences and Social Norms of the MacArthur Foundation, and the Richard Büchner-Stitung is gratefully acknowledged.

Editor's note: This paper was accepted by Madhav Rajan.

Submitted October 2004
Accepted August 2006

the manager can either prepare correct financial statements or overstate the economic situation of his company. The auditor can either conduct a cursory audit that leaves manipulations undetected, or a comprehensive audit. If all players act purely opportunistically, i.e., choose their strategies based only on their economic incentives, then the solution of such a game is a unique Nash Equilibrium in mixed strategies. The manager's and auditor's equilibrium strategies, in turn, determine the quality of audited financial reports. In the following, we address the question of how to decrease the frequency of manipulations the auditor fails to detect.

Standard game theory for the models mentioned above predicts that substituting some of the opportunistic auditors with auditors who always choose high audit effort and report their findings truthfully to the public does not affect the equilibrium quality of audited financial statements. The following arguments lead to this result. If auditors who always perform a high audit effort replace some opportunistic auditors, the remaining opportunistic types completely offset their reputable colleagues' outstanding audit quality. Because the manager's expectation of overall audit quality (comprising the unconditionally honest auditors) does not change, he has no incentive to adjust his reporting strategy.

In contrast to the above, the present paper describes a laboratory experiment showing that an increase in the proportion of auditors who always perform high audit effort irrespective of their economic incentives indeed increases the quality of audited financial statements. This result was obtained even though the payoffs in the bimatrix game we investigated remained constant. In particular, we implemented two treatments representing two extreme scenarios in the relative proportion of auditor types. In treatment A, all participants in the role of auditors chose their audit effort, which became relevant for the payment participants received at the end of the experiment. In treatment B, we randomly substituted *ex post* one-third of the auditor participants' decisions on audit effort with a high audit effort. Thus, computerized auditors who automatically exerted high audit effort performed one-third of the audits in treatment B. By comparing the participants' behavior between treatments, we analyzed the computerized auditors' effect on both the auditor participants' audit effort and on the manager participants' reporting choice.

In line with standard theory, we find that the presence of computerized auditors induces participants in the role of auditors to choose a high audit effort less often. Thus, computerized auditors, which automatically exert high effort, crowd out auditor participants who perform high effort strategically. Nevertheless, and contrary to the Nash prediction, the overall audit quality is actually higher in the treatment with computerized auditors, because the crowding out is incomplete. Thus, managers manipulate financial statements less often when faced with a certain percentage of computerized auditors, because there is a greater probability of manipulated reports being detected than there is without computerized auditors. Therefore, the quality of audited financial reports is indeed higher if computerized auditors are present. With regard to the current policy debate on audit oversight, which in addition has payoff consequences, our results suggest that the increase in type uncertainty caused by a "random audit of audits" by itself improves the quality of audited reports.

We show that the deviations of individual behavior from Nash predictions that we observed in our experiment can be modeled with boundedly rational behavior, as captured by the Quantal Response Equilibrium developed in McKelvey and Palfrey (1995). This model can be envisaged as one where players make an error in the assessment of their expected payoff. Such errors are relevant in the game we consider, because even small errors in the assessment of the other player's behavior create a strong incentive to diverge from the equilibrium strategy in a mixed Nash Equilibrium. Subjects' risk aversion does not account for our findings. Robustness checks indicate that neither the framing of the experiment in an auditing context nor the specific payoff matrix we implemented accounts for our results.

Our results are applicable to auditing, provided the proportion of auditors who always exert high audit effort could actually increase, even without the introduction of additional sanctions on professional misconduct. This assumption is justified if the results of general experiments on behavioral economics are also true for auditors. First, Gneezy (2005) demonstrated in a generic experiment that at least some people are intrinsically trustworthy, because they dislike deceiving others and therefore prefer telling the truth, even if this alternative has negative economic consequences. In line with his results, we argue that there are also CPAs who conduct high-quality audits in accordance with the relevant standards, and who report their findings truthfully, even though their profits might otherwise have been greater. Moreover, Pruckner and Sausgruber (2006) showed in a field experiment that there are people whose trustworthiness depends on how the decision problem is presented to them. Pruckner and Sausgruber (2006) tried to activate the social norm to be honest by printing "Thank you for being honest!" on the sales booths a publisher uses for selling newspapers on the streets. Compared to a situation without an emphasis on socially desirable behavior, this treatment increased the average payment of one subgroup of subjects (the conditionally honest people), while that of the other subgroup (the purely opportunistic types) remained unaffected. Translated into the audit market, this result means that provided there are auditors with social preferences for honesty and trustworthiness, their audit quality could be improved by emphasizing professional standards for ethical behavior, which can be interpreted as activating a social norm. Therefore, the factual percentage of reputable auditors can increase if the importance of professional ethics is stressed, whereas managers cannot observe the auditor's preferences for honesty *ex ante*.

The paper proceeds as follows. In Section II we illustrate the auditor-manager game that serves as the basis for our experiment, discuss related experimental evidence, and outline the motivation for our investigation. In Section III we describe the design of the experiment in detail. Section IV discusses standard predictions for risk-neutral and risk-averse subjects, introduces the concept of the Quantal Response Equilibrium, and derives corresponding hypotheses. Section V contains the results of our basic experiment and of our robustness checks, and Section VI summarizes and concludes.

II. BACKGROUND AND MOTIVATION

Matching Pennies Games between a Manager and an Auditor

We focus on a game between two players, where each player can choose one of two available actions. We concentrate on a one-shot matching pennies game, which has a single Nash Equilibrium (NE) in mixed strategies as a solution. The model of Magee (1980), which describes the quality of audited financial statements as the result of the strategic auditor-manager interaction,[1] as well as the papers by Fellingham and Newman (1985) and Shibano (1990), which analyze the assessment of audit risk,[2] serve as examples of matching pennies games in an auditing context.

In the game we consider, the manager of a company can choose between two alternatives: to disclose financial statements that truthfully reflect the firm's economic condition, or to manipulate financial statements in such a way as to overstate the underlying economic situation of the company. After the manager has made his decision, the auditor, who cannot observe the manager's actual decision, also has two choices. He can either exert high audit effort that perfectly detects all errors and irregularities that will eventually be contained in the financial statements, or low audit effort, which will leave manipulations undetected. We assume that the auditor renders an adverse opinion if he has observed that the manager has manipulated financial statements, and that he issues a clean opinion otherwise.[3]

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[1] Smith et al. (2000) extend the model sequentially by offering the auditor the choice of auditing the internal control system before a matching pennies game is played. Anderson and Young (1988) use a similar game for planning internal audits. Matsumura and Tucker (1995) as well as Tucker and Matsumura (1997) analyze second-partner-reviews with a matching pennies game.

[2] For extensions of the basic model, see Fellingham et al. (1989), Newman and Noel (1989), Patterson (1993), Bloomfield (1995), and Newman et al. (2001).

The manager earns a basic salary if he reports truthfully. If he overstates the firm's financial condition and the auditor detects this misreporting, then the manager incurs additional costs. These costs can result from a loss of reputation, a decrease in compensation based on accounting numbers, or adverse reactions of investors or creditors. If the auditor fails to detect the overstatement, then the manager earns a bonus payment, which may not be justified by his effort.

The auditor's basic profit equals a fee that is adequate for choosing a high audit effort less the costs he incurs for exerting low effort. If the auditor chooses a high effort, then supplementary audit costs must be deducted from his basic profit. The auditor suffers a loss (e.g., reputational damages or legal action initiated by third parties) if he exerts low audit effort and issues a clean opinion when an adverse opinion would have been appropriate. If the auditor publicly reports any detected manipulations, then he risks losing the client and thus the economic advantages derived from the continuation of the auditor-client relationship, referred to as quasi rents (DeAngelo 1981; Kanodia and Mukherji 1994; Magee and Tseng 1990; Lee and Gu 1998; Schatzberg 1994; Schatzberg and Sevcik 1994). In this case, reputational gains could provide an incentive for the auditor to signal to the public that he has exerted high audit effort.

Since the auditor cannot observe the action the manager has actually taken before performing his audit, and the manager cannot infer the audit effort undertaken before he chooses his report, we can analyze the game as if the players acted simultaneously. If we make the standard assumptions of risk neutrality, perfect rationality, and common knowledge about these attributes, then the payoff structure of a matching pennies game implies that there is a single NE in mixed strategies in which the manager overstates the economic condition of the firm with probability $\alpha^* \in (0,1)$, and the auditor chooses a low audit effort with probability $\beta^* \in (0,1)$.

$$1 - \phi^* = 1 - \alpha^* \cdot \beta^* \tag{1}$$

defines the quality of audited financial reports, where $\phi^* \in (0,1)$ characterizes the probability that financial statements presented to third parties contain irregularities or overstatements of the firm's economic condition that the auditor has failed to detect. The quality of audited financial statements is thus the result of the strategic auditor-manager interaction.

Related Experimental Studies

Experimental studies conducted in more general economic settings show that actual human behavior correlates well with the NE if certain conditions are fulfilled. However, there are situations in which the experimental data deviate strongly from standard predictions. Standard theory assumes that individuals have the ability to behave perfectly rationally, and this presumption is generally used to explain the latter observation. The predictive power of the NE seems to depend on the following circumstances.

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[3] The problem of auditor independence is not the focus of the current paper. For work on independence, see DeAngelo (1981), Antle (1984), Magee and Tseng (1990), Dye (1991), or Lee and Gu (1998).

First, laboratory experiments frequently confirm the point prediction of the NE of a symmetric matching pennies game in which players choose the available actions with equal probabilities. However, experimental data for similar games with asymmetric choice probabilities rarely support the Nash prediction (Goeree and Holt 2001).

Second, in an audit experiment, Bloomfield (1997) demonstrates that the slope of the best-reply functions (i.e., the degree of strategic dependence) is crucial for the predictive power of the NE. Bloomfield (1997) considers exogenous errors that stem from an auditor's uncertainty on the manager's payoffs from undetected fraud, a positive probability for unintentional errors in the account balance, as well as a normally distributed signal, which is informative of the quality of the account balance. As a result, the best-reply functions of the audit game tested by Bloomfield (1997) are not right-angled, as the best-reply correspondences in the simple matching pennies game are. Bloomfield (1997) shows that the NE predicts human behavior more precisely if strategic dependence is low, i.e., if the best-reply functions are not too steep.

Third, if the standard prediction does not seem evident at first sight, the NE often cannot explain changes in human behavior that occur after the parameters of the game have been varied. For example, consider the payoff changes induced by Section 304 of the Sarbanes-Oxley Act. CFOs and CEOs of SEC registrants are now required to reimburse their company for any bonuses or other incentive- or equity-based compensation if the firm must prepare an accounting restatement due to material noncompliance. In our game, the manager's expected payoff if he misreports is thus lower if the auditor does not change his effort level. For the class of games we consider, the player's NE strategy depends exclusively on the other player's payoffs, not on his own payoff. Therefore, to make the manager indifferent between his pure strategies again, the auditor should respond to the change in the manager's payoffs with a lower probability for a high audit effort. The manager's behavior should remain constant.

Nevertheless, experimental data clearly contradict the comparative static effect predicted by standard theory, because both subjects adjust their behavior in a very intuitive way after a payoff variation for one subject has occurred (Goeree and Holt 2001; Goeree et al. 2003). This "Own Payoff Effect" persists even in experiments with repeated games (Ochs 1995; Goeree et al. 2000; McKelvey et al. 2000). Therefore, we might observe not only less frequent choices of high audit effort, but also a lower frequency of manipulations if we tightened the manager's sanctions.

Motivation

Although there is ample empirical evidence on the manager's and auditor's payoff components in the game we consider, detailed knowledge of the determinants of actual behavior observed in strategic auditor-manager settings remains limited. The latter observation is especially noticeable for those games where strategic dependence is high and where the NE often fails to predict actual behavior correctly (Kachelmeier 1991; Dopuch and King 1992, 1996; Bloomfield 1994, 1997). This gap in knowledge is the motivation for our analysis of the factors that determine behavior in matching pennies games in an auditing context.

First, the question of whether individuals' behavior correlates with the Nash prediction in static settings of the auditor-manager game inherently has practical relevance. To minimize audit risk by choosing the most efficient audit effort level for a given client, the auditor needs a precise estimation of inherent risk. If we interpret the decision-theoretic audit risk model in a game-theoretic context (Fellingham and Newman 1985; Shibano 1990; Bloomfield 1995), then inherent risk is endogenously determined by the strategic behavior of the manager. The manager, in turn, chooses his reporting strategy contingent on his estimation of the strategic effort his auditor chooses to optimize detection risk.

Second, we are interested in predicting behavioral changes as a consequence of the variation of certain conditions of the audit environment. According to standard theory, comparative static effects for auditors' and/or managers' behavior can result not only from payoff variations, but also from adding or removing type uncertainty. Other studies have documented the Own Payoff Effect outlined above in generic experiments, and we do not expect fundamentally different results in an audit experiment. Since the effect of manager's uncertainty on the auditor's type has not yet been tested, we investigate the impact of changes in the relative proportion of opportunistic and reliable auditors on the quality of audited financial statements. As a robustness check, we also varied the auditors' payoffs implemented in our basic experiment. As a side effect, our data confirm the existence of an Own Payoff Effect as documented in the related literature.

III. DESIGN OF THE AUDIT EXPERIMENT

In our experiment, half of the subjects played the role of the manager and the other half that of the auditor. Subjects were randomly assigned their roles, which they maintained for the whole session. After all subjects had read the instructions for their respective roles, they answered control questions. The experiment began after we had orally summarized the instructions for both managers and auditors, including the whole payoff matrix. We gave this detailed summary to ensure that all participants knew that both sets of instructions defined the same set of rules.

In total, we conducted 15 sessions, each with a different set of participants, and implemented two treatments, A and B, in each session. In treatment A, all decisions made were used as a basis for calculating the participants' compensation earned during the experiment. Thus, treatment A consisted entirely of completely "opportunistic" managers and auditors, because subjects chose the strategy they assumed would maximize their expected payoffs. In treatment B, however, we randomly selected one-third of the decisions of all participants in the role of auditors and automatically substituted these decisions on audit effort *ex post* with a high audit effort.[4] All subjects knew this process and thus that there was a one-third probability for a computerized auditor in treatment B. All of the managers continued to behave opportunistically in treatment B. Each treatment consisted of 20 periods in which the auditor-manager game described below was played.

We implemented the treatments consecutively. We conducted five sessions in which the participants first interacted in accordance with treatment A and then with treatment B. To control for order effects, we ran five further sessions in the opposite sequence, i.e., treatment B was followed by treatment A. Participants knew which treatment was in effect at any given point.

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[4] We chose a one-third probability of computerized auditors for the following reasons: For our basic payoff matrix, the percentage of computerized auditors does not alter the standard prediction for managers' behavior as long as there are not more than 50 percent computerized auditors. Nevertheless, the shift in the manager's Quantal Response function, explained later on, is more pronounced for a higher percentage of computerized auditors. Thus, we probably would have observed larger differences between the treatments if we had chosen a higher probability for computerized auditors. The implemented proportion of 33.33 percent computerized auditors seems large enough for the participants to notice the difference between the treatments, but not so large as to give the NE too remote a chance.

In all sessions of the basic experiment, except the first, we formed three matching groups. Because a matching group is a subset of participants in a session to which interaction is restricted, and there is no interaction between subjects of different matching groups, observations in different matching groups are statistically independent. We randomly matched one auditor with one manager from the same matching group in each period of a treatment.

Table 1 describes our sessions in detail. Session numbers 1–10 refer to the basic experiment (payoff matrix 1). Table 1 also shows sessions 11–15 described later on, which we conducted to test the robustness of our results. A total of 394 students at two large universities participated in the experiment. We programmed and conducted the experiment using the z-Tree software (Fischbacher 1999).

TABLE 1
Description of the Experimental Sessions

Session Number	Treatment Order[a]	Framing[b]	Payoff Matrix[c]	Number of Participants	Number of Matching Groups[d]
1	A–B	auditor/manager	1	24	1
2	A–B	auditor/manager	1	24	3
3	A–B	auditor/manager	1	22	3
4	A–B	auditor/manager	1	24	3
5	A–B	auditor/manager	1	22	3
				Subtotal: 116	
6	B–A	auditor/manager	1	24	3
7	B–A	auditor/manager	1	24	3
8	B–A	auditor/manager	1	24	3
9	B–A	auditor/manager	1	24	3
10	B–A	auditor/manager	1	24	3
				Subtotal: 120	
11	A–B	neutral	1	32	4
12	A–B	neutral	1	32	4
				Subtotal: 64	
13	A–B	auditor/manager	2	32	4
14	A–B	auditor/manager	2	32	4
15	A–B	auditor/manager	2	30	4
				Subtotal: 94	
				Total: 394	

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[a] This column specifies the order in which we conducted treatment A (only opportunistic auditors) and treatment B (one-third computerized auditors).

[b] We used terms in the auditing context in the experimental instructions for the sessions with auditor-manager framing. Neutral framing means that we avoided terms from the auditing context in the instructions.

[c] We define the payoff matrices we used in the experiment in Table 2.

[d] The number of matching groups indicates how many subsets of participants to which interaction is restricted were formed among the subjects of each session.

In each period, we asked manager participants whether they wanted to disclose financial statements that truthfully reflected the firm's economic condition, or if they preferred overstating the underlying economic situation of their company. The auditor participants had to choose whether to exert low or high audit effort.[5] We did not inform participants of their partners' decisions before they made their own choices.

Table 2 shows the payoffs in points that could be earned in each period of the basic experiment (payoff matrix 1) as well as in the additional experiment with payoff matrix 2. After each participant had chosen one of the possible actions, we told both managers and auditors about their counterparts' decisions and their own corresponding earnings. If the experiment software had selected a computerized auditor to substitute for a human auditor in treatment B, then the manager only learned that high audit effort had been exerted automatically. We did not tell the manager about the decision that the auditor participant had actually chosen.

At the end of the experiment, we converted the points earned into money (one point is roughly equivalent to 5 cents, calculated in U.S. dollars) and paid this amount, together with the approximately $6.09 show-up fee to the participants, anonymously. On average,

TABLE 2
Payoff Matrices Used in the Experiments (Payoffs in Points)

Basic Experiment (Payoff Matrix 1, Auditor-Manager Framing)[a]

Manager Auditor	Correct Financial Statements	Overstatement of Economic Condition
Comprehensive audit	6 2	3 8
Normal audit effort	6 7	9 6

Additional Experiment (Payoff Matrix 2, Auditor-Manager Framing)

Manager Auditor	Correct Financial Statements	Overstatement of Economic Condition
Comprehensive audit	6 5	3 8
Normal audit effort	6 7	9 3

[a] In sessions 11 and 12 with neutral framing and payoff matrix 1, we denoted the manager and the auditor as "participant A" and "participant B," respectively, and labeled the actions as "X," "Y," "I," and "II."

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[5] We used the terms "normal audit effort" (instead of "low audit effort") and "comprehensive audit" (instead of "high audit effort") in the instructions and did not use value-laden terms for describing the difference between treatments to avoid encouraging our subjects to exert high effort to obey a social norm eschewing low effort.

subjects earned about \$18.02 for a session of approximately one hour. The compensation received is roughly equivalent to the wages our participants would have earned if they had taken another opportunity on the job market.

IV. HYPOTHESES

Standard Predictions

In a situation with a percentage $r \in (0, 1 - \beta^*)$ of computerized auditors, managers have only incomplete information about their auditor's type. The solution of the game again is a single NE in mixed strategies, i.e., both the manager and the opportunistic auditor take probabilistic choices of their respective pure strategies. The standard prediction for the probability that an auditor chooses a low audit effort is given by:

$$\beta^{**} = \beta^* \cdot \frac{1}{1 - r}, \tag{2}$$

because $(1 - r) \cdot \beta^{**} = \beta^*$, which determines the behavior of the remaining opportunistic auditors, must hold. Since $\beta^{**} \geq \beta^*$ is true, the NE predicts that the remaining opportunistic auditors will choose a low audit effort more often if there are computerized auditors.

$(1 - r) \cdot \beta^{**} = \beta^*$ indicates that the overall quality level of audits remains constant, because the opportunistic auditors completely offset the decisions of the computerized auditors. Moreover, managers anticipate that opportunistic auditors adjust their behavior perfectly and therefore do not have an incentive to deviate from the way financial statements were prepared before the entry of computerized auditors. Therefore, the probability that financial statements will be manipulated is again given by:

$$\alpha^{**} = \alpha^*. \tag{3}$$

We recall that computerized auditors never choose a low audit effort. Therefore, as long as there is only a relatively small number of computerized auditors (i.e., $r \leq 1 - \beta^*$), the overall quality of audited reports is given by:

$$1 - \phi^{**} = 1 - (\alpha^{**} \cdot (1 - r) \cdot \beta^{**}) = 1 - \alpha^* \cdot \beta^* = 1 - \phi^*. \tag{4}$$

Thus, according to the standard theory, the existence of only a low number of computerized auditors should not alter the overall quality of audited financial statements that will evolve in a market of purely opportunistic players.

For the payoffs in our basic experiment (Table 2), the NE in mixed strategies predicts that managers will manipulate financial statements with probability $\alpha^* = \alpha^{**} = 0.714$ in both treatments. Opportunistic auditors are predicted to exert low audit effort with probability $\beta^* = 0.5$ in treatment A and $\beta^{**} = 0.75$ in treatment B. Thus, the equilibrium quality of audited reports can be calculated to $1 - \phi^* = 0.643$ in treatment A. The NE predicts the same quality of audited financial statements for treatment B ($1 - \phi^{**} = 0.643$), given $r = 1/3$.

Risk Preferences and Nash Equilibrium

The predictions of the equilibrium probability choices given above are only valid if subjects are risk-neutral. However, if expected utility is considered, we can regard risk preferences as a monotonic transformation of players' payoffs. Therefore, the structure of the game remains unchanged, even if we take risk preferences into account. Thus, auditors are indifferent between high and low audit effort at a certain manipulation probability that does not differ between treatments. Therefore, the equilibrium prediction for the manager is the same in both treatments, although risk preferences determine the actual value predicted for $\alpha^* = \alpha^{**}$.

Quantal Response Equilibrium

The observation that the NE fails to predict actual behavior is generally ascribed to the fact that individuals do not have the ability to behave perfectly rationally. The Quantal Response Equilibrium (QRE) (McKelvey and Palfrey 1995) is a concept that explicitly considers boundedly rational behavior and thus allows for qualitative, testable behavioral predictions.

We give a formal presentation of the theory, which has been successfully applied to explain experimental data for various classes of games (Goeree et al. 2000),[6] in the Appendix. The intuition behind this concept is that players' decisions are not only subject to errors in a very general sense, but that players also take the others' errors into account.[7] Thus, players do not always choose their best strategy, but the probability of playing a strategy increases in the payoff of that strategy choice, given the other player's behavior. The strategy that results from this noisy optimization process is called the Quantal best reply. It is a mixed strategy that is conditional on the other player's strategy. The QRE is a fixed point of mutual Quantal best replies.

Depending on the magnitude of the error, the predictions based on the QRE can deviate fundamentally from the Nash prediction. It is legitimate to question whether the introduction of a seemingly arbitrary error term is justified. This point is of particular importance, as the QRE is not based on an explicit specification of the errors' psychological nature, and, moreover, does not provide insight into the psychology of the decision process itself. In addition, one can challenge if the QRE is appropriate for explaining individual behavior in the auditing context.

Our opinion is that the answer to both questions is affirmative for the following reasons. First, we argue that strategic uncertainty is a main source of boundedly rational behavior. Assume a game with a high degree of strategic dependence, i.e., a situation in which the optimal choice of player A's pure strategy switches at a critical probability for player B's mixed strategy. If A does not know precisely how B actually will decide, then A cannot correctly compute both his own expected payoff as well as that of B. Thus, decision errors in the sense of imperfect information on the other player's strategy choice might induce a deviation of actual behavior from the Nash prediction. Strategic uncertainty is particularly relevant also in the auditing context, because the manager's and auditor's actions are closely interrelated. In reality, we can assume that neither the auditor can precisely anticipate inherent risk (i.e., the manager's strategic reporting choice) nor can the manager predict detection risk (i.e., the auditor's strategic audit effort). In addition, both the manager and the auditor might not know the concrete value of their own payoffs and/or those of the other player with certainty. For example, the manager probably knows neither the exact value of his reputational losses nor the monetary equivalent of disadvantageous shareholders' and creditors' reactions he might incur if he misreports. For the auditor, the payoff equivalent of reputational gains, the monetary consequences of losing credibility with third-parties, and of the quasi rents at risk might be unknown. Moreover, the actual proportion of reputable auditors is unobservable in real-world settings. The inclusion of errors allows for players' optimization based on noisy payoffs and/or conditions of the audit market.

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[6] Anderson et al. (1998), Capra et al. (1999) as well as Anderson et al. (forthcoming) used the Quantal Response Equilibrium for the case of extreme value distributed errors (Logit Equilibrium). Moreover, Goeree and Holt (2001) give an overview of different classes of games (static and dynamic games, each with complete and incomplete information) played only once. For each game considered, experimental data are perfectly in line with Nash predictions for a specific parameter specification (treasure treatment). However, actual behavior diverges dramatically from the Nash Equilibrium for another contradiction treatment. Using the Logit Equilibrium, predictions that fit remarkably well with experimental data can be derived for both parameter sets considered.

[7] Weizsäcker (2003) has developed an extension to the theory we present, which allows distinguishing between the errors people actually make and their beliefs about the errors of others. He shows in an experiment that people overestimate the errors of the other subjects.

Second, one can interpret the error as a measure for the heterogeneity in decision maker's preferences, which might be unobservable for the other player. This feature is also characteristic of real auditor-manager interactions, because neither the manager nor the auditor is informed about the other's actual risk preferences. In addition, the manager cannot observe his auditor's preferences to align strongly with professional guidelines, nor can the auditor precisely infer the manager's incentives to manipulate before he actually has conducted the audit.

In our experiment, a participant's uncertainty about the other subject's action choice as well as the heterogeneity in decision-makers' preferences are the most important sources of decision errors in the sense of the QRE. However, the main strength of the QRE is that it makes accurate qualitative predictions even if both actual preferences and the psychological factors that are responsible for observed behavior are unknown.

Hypotheses Based on the Quantal Response Equilibrium

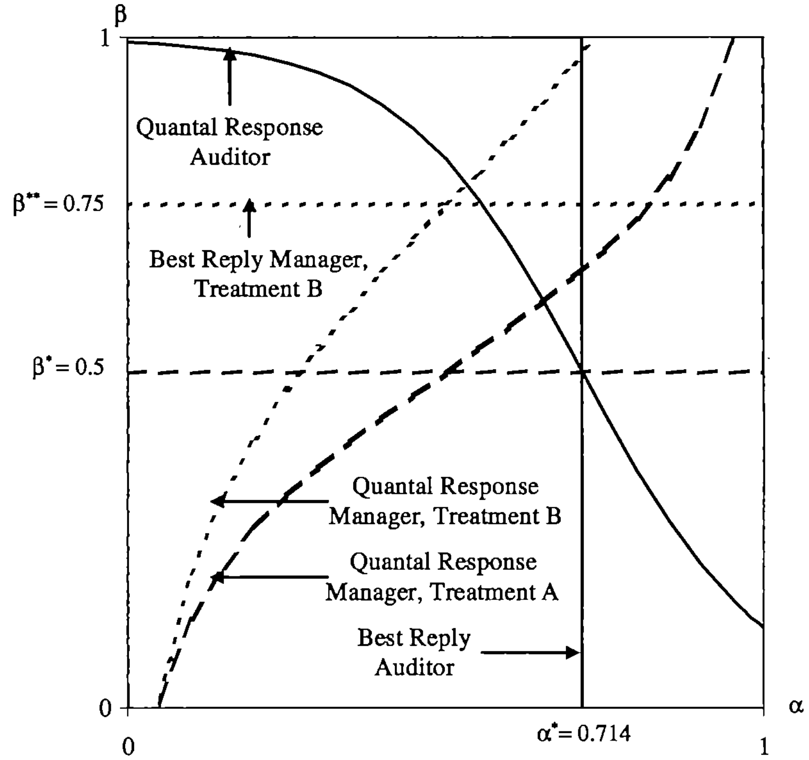
Taking errors into account leads to quite different behavioral predictions in our experiment than based on the NE. In Figure 1, we illustrate how the Quantal Responses correspond to the best replies of the NE for the payoffs of the basic experiment and an error parameter of $\mu = 1$.

In a market that consists purely of opportunistic auditors, according to the NE, the auditor switches from low to high audit effort at the critical manipulation probability of $\alpha^* = 0.714$. In the Quantal Response function, the auditor gradually lowers the probability of a low audit effort with an increasing manipulation probability. Similarly, the manager is more likely to manipulate as the probability of a low audit effort increases. In our example, the QRE for treatment A predicts that managers misreport less often and opportunistic auditors exert low audit effort more often than we would expect if we applied standard theory.

Contrary to the NE, the QRE can capture the intuition that managers manipulate financial statements less frequently if there are a few computerized auditors. Because we use the same payoffs for both treatments, the opportunistic auditor's Quantal Response function does not differ between the treatments. Nevertheless, the introduction of computerized auditors (treatment B) shifts the manager's Quantal Response function to the upper left. Since the auditor's Quantal Response function is monotonically decreasing in α, we can make the unambiguous and therefore disprovable prediction of a higher probability for a low audit effort and a lower probability for manipulations in treatment B compared to treatment A, i.e., a shift of the QRE to the upper left (see Figure 1). Thus, we formulate a hypothesis about the change in opportunistic managers' behavior that contradicts the standard prediction:

H1: Managers manipulate financial reports less frequently in treatment B than in treatment A.

FIGURE 1
Best Replies and Quantal Responses for Treatments A and B
Payoff Matrix 1 and Error Parameter $\mu = 1$



Variable Definitions:
α = probability for manipulations; and
β = probability for a low audit effort.

Concerning the effects on auditors' behavior, we need to distinguish between the opportunistic auditors' behavior and the resulting overall audit quality, which is determined also by the computerized auditors. Standard theory predicts the same overall audit quality for both treatments, because opportunistic auditors choose a low audit effort more often in the presence of computerized auditors and thus exactly offset the automatically high audit effort. In the following hypotheses derived from the QRE, H2 does not discriminate between standard theory and the QRE, but H3 does:

H2: Opportunistic auditors exert low audit effort more often in treatment B than in treatment A.

H3: Overall, auditors perform fewer low effort audits in treatment B than in treatment A.

Hypothesis 2 means that opportunistic auditors offset the effect that computerized auditors have on audit quality, i.e., computerized auditors crowd out auditor participants who strategically choose a high audit effort. Hypothesis 3 states that this crowding out is incomplete in the sense that overall audit quality increases if computerized auditors are present. Thus, H3 contradicts standard theory.

Hypothesis 3 clarifies the intuition of why we expect managers to manipulate less often if there are computerized auditors. In standard theory, opportunistic auditors offset the decisions of the computerized auditors completely, in the sense that managers face the same detection risk in both treatments. However, if auditor participants make errors, then they only compensate incompletely for the computerized auditors. Therefore, managers actually face a higher detection risk with computerized auditors, and thus manipulate less often.

Authors such as Haile et al. (2003) criticize the concept of the QRE, since it allows arbitrary predictions in a normal form game. This criticism does not apply to our experiment for the following reason: since the auditor's Quantal Response does not change between the treatments, the predictions that managers manipulate less often and auditors provide low effort audits more frequently in treatment B than in treatment A are independent of the concrete value for the error parameter μ, as long as $\mu > 0$.

V. RESULTS

Result 1: Auditor participants exert low audit effort more often in the presence of computerized auditors.

In the upper left-hand panel of Figure 2, we graph the frequency of low audit effort performed by the auditor participants in the two treatments over time. Auditors exert low effort more frequently if computerized auditors are present. The difference we observe is statistically significant. In all sessions, more auditors exert low audit effort in treatment B (with computerized auditors), compared to treatment A (without computerized auditors). This result is completely in line with the predictions of both standard theory and the QRE.

We compare the auditors' decisions within matching groups across treatments by using a Wilcoxon signed-rank test. We apply this test to all data, and to the two session types differing in the treatment order. Table 3 shows that the difference between the frequencies of low audit effort across treatments is highly significant, irrespective of the treatment order. Furthermore, a Mann-Whitney test shows that if we compare behavior between the treatments that we conducted first in the treatment order (i.e., we compare treatment A of the sessions 1–5 with treatment order A–B to treatment B of the sessions 6–10 with treatment order B–A), then the difference is also highly significant at the 0.01 level. Finally, we perform a rough test for time dependence by looking only at the data of periods 11–20. We also observe the predicted difference in the second half of the experiment. Thus, the data confirm H2.

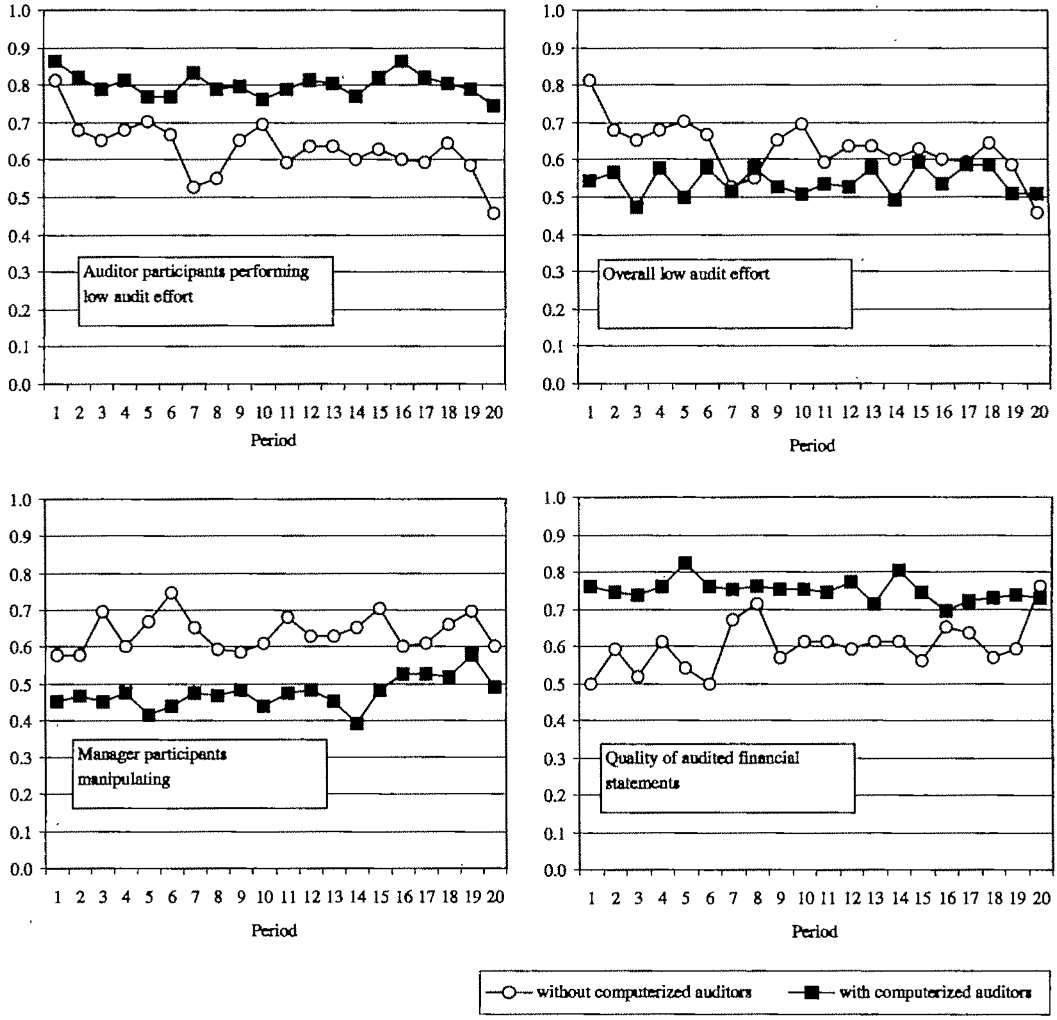
This result is not surprising and it completely accords with the standard prediction. If some auditors automatically choose a high audit effort, then the subjects in the role of auditors are predicted to compensate. In H3, we conjecture that this crowding-out is incomplete, in the sense that the overall audit quality in treatment B is higher than in treatment A:

Result 2: Auditor participants only incompletely compensate for the decisions of the computerized auditors.

The upper right-hand panel of Figure 2 graphs the overall probability of low effort audits from the managers' perspective. As predicted by H3, we observe low effort audits less often in treatment B with computerized auditors.

However, the difference in overall audit quality between treatments shrinks over time, which we interpret as learning by auditor participants. Table 3 shows that the difference in

FIGURE 2
Frequencies of Auditor's Low Audit Effort, Overall Low Audit Effort, Manager's Manipulations, and the Quality of Audited Financial Statements over Time



the overall audit quality for periods 11–20 is only significant in the sessions in which the computerized auditors were introduced in the second treatment (treatment order A–B, sessions 1–5). It seems that only in this case was learning slow enough to preserve a significant difference.

In Table 4 we also study a probit model with the actual overall audit effort as the dependent variable for this particular case. Our main independent variables are a dummy variable for treatment B (*IB*) and the period number minus 20 (*P*−), so we can interpret the constant and dummy variables as the effect at the end of the experiment. We see that the dummy for treatment B is not significant. Thus, there is no statistical difference in the

TABLE 3
Statistical Tests for Significant Differences across Treatments A and B

	Tests	Data[a]	All Periods	Periods 11–20
Δ*Low Audit*	Wilcoxon signed-rank test	All data	4.555***	4.362***
		Sessions 1–5	3.110***	2.830***
		Sessions 6–10	3.352***	3.324***
	Mann-Whitney test	First treatments	4.015***	3.490***
Δ*Overall Low Audit*	Wilcoxon signed-rank test	all data	−4.158***	−1.062**
		Sessions 1–5	−3.111***	−2.521**
		Sessions 6–10	−2.814***	−0.369
	Mann-Whitney test	First treatments	−2.887***	−1.342
Δ*Manipulation*	Wilcoxon signed-rank test	All data	−3.952***	−3.747***
		Sessions 1–5	−2.760***	−2.411**
		Sessions 6–10	−2.813***	−2.841***
	Mann-Whitney test	First treatments	−3.759***	−3.527***
Δ*Quality*	Wilcoxon signed-rank test	All data	4.555***	3.976***
		Sessions 1–5	3.181***	2.797***
		Sessions 6–10	3.239***	2.704***
	Mann-Whitney test	First treatments	4.079***	3.732***

*, **, *** Indicate statistical significance at 10 percent, 5 percent, and 1 percent, respectively (two-tailed tests).
This table reports statistical tests for the behavioral differences across treatments. It shows that the hypothesized effects are highly significant in almost every specification. We used Wilcoxon signed-rank tests for comparing behavior across treatments within sessions and Mann-Whitney tests for comparing behavior in the first treatment across sessions. Matching group averages are the independent observations. The number shown in the table represents the z-value. It is positive if the corresponding variable is higher in the treatment with computerized auditors.

[a] The comparison of the treatments conducted first in the treatment order across sessions is called "first treatments."

Variable Definitions:
Δ*Low Audit* = difference between the frequencies of low audit effort across treatments that auditor participants chose;
Δ*Overall Low Audit* = difference between overall low audit effort, which also comprises the computerized audits, across treatments;
Δ*Manipulation* = difference between managers' decisions to manipulate across treatments; and
Δ*Quality* = difference between the resulting quality of audited financial statements across treatments.

overall audit effort between the treatments at the end of the sessions for treatment order B–A.

We check to see why the computerized auditors have no significant effect on the resulting overall audit effort in the later periods of the reversed treatment order B–A. The audit effort exerted in treatment A (without computerized auditors) is similar in both sequences (A–B and B–A). The treatment effect that we observe for the overall low audit effort happens because the introduction of computerized auditors (treatment order A–B) reduces the overall low audit effort by almost 10 percent (from 0.605 to 0.507), even in periods 11–20. The disappearance of computerized auditors (treatment order B–A) does not increase the overall low audit effort to the same extent, which we also see in Table 5.

This result has an important policy implication. If the introduction of reputable auditors has a stronger effect than their removal, then even a temporary increase in the proportion of honest auditors (e.g., as a consequence of stressing professional standards) could lead to a permanent improvement of audit quality.

TABLE 4
Probit Regressions with Auditor's Decision, Manager's Decision, and Quality as Dependent Variables

	Low Audit		*Overall Low Audit*		*Manipulation*		*Quality*	
Periods	**1–20**	**11–20**	**1–20**	**11–20**	**1–20**	**11–20**	**1–20**	**11–20**
Constant	0.723	0.709	0.586	0.571	0.557	0.570	0.680	0.682
IB	0.225	0.230	−0.011	−0.012	−0.137	−0.104	0.081	0.067
	(0.039)***	(0.043)***	(0.035)	(0.039)	(0.038)***	(0.036)***	(0.031)***	(0.028)**
P–	−0.007	−0.008	−0.008	−0.010	0.001	−0.002	0.005	0.008
	(0.002)***	(0.004)**	(0.002)***	(0.005)**	(0.001)	(0.003)	(0.002)***	(0.004)*
IB · (*P–*)	0.006	0.006	0.008	0.009	0.003	0.011	−0.007	−0.012
	(0.004)	(0.006)	(0.003)***	(0.007)	(0.002)	(0.004)***	(0.003)***	(0.006)*
χ^2	94.48	63.84	52.41	10.24	49.13	27.49	68.71	24.59
Prob > χ^2	0.000	0.000	0.000	0.017	0.000	0.000	0.000	0.000
Pseudo R^2	0.035	0.043	0.009	0.003	0.021	0.019	0.022	0.014

*, **, *** Indicate statistical significance at 10 percent, 5 percent, and 1 percent, respectively (two-tailed tests).

This table reports probit regressions for the auditors' decisions to exert low audit effort, the overall low audit effort, the managers' decisions to manipulate, and the resulting quality of audited financial statements as dependent variables. By using the variable *P–* (period minus 20) as a regressor, the treatment dummy *IB* measures the estimated treatment effect in period 20. We report marginal effects; robust standard errors with the matching groups as clusters are in parenthesis. We also run all regressions with data only from periods 11–20.

Variable Definitions:

Low Audit = frequency of the low audit effort that auditor participants chose;
Overall Low Audit = overall low audit effort, which also comprises the computerized audits;
Manipulation = frequency of manipulated financial statements that manager participants chose;
Quality = resulting quality of audited financial statements;
IB = B treatment (dummy); and
P– = period minus 20.

TABLE 5
Average Decisions of Auditors and Managers in the Two Treatment Conditions

	Low Audit		*Overall Low Audit*		*Manipulation*		*Quality*	
Session[a]	Treatment A	Treatment B	Treatment A	Treatment B	Treatment A	Treatment B	Treatment A	Treatment B
1	**0.51**	0.65	**0.51**	0.46	**0.67**	0.57	**0.68**	0.75
2	**0.66**	0.85	**0.66**	0.55	**0.67**	0.49	**0.56**	0.72
3	**0.63**	0.80	**0.63**	0.53	**0.69**	0.42	**0.57**	0.79
4	**0.69**	0.84	**0.69**	0.55	**0.67**	0.48	**0.54**	0.74
5	**0.63**	0.76	**0.63**	0.52	**0.70**	0.52	**0.54**	0.74
6	0.55	**0.82**	0.55	**0.58**	0.61	**0.47**	0.67	**0.72**
7	0.63	**0.78**	0.63	**0.53**	0.61	**0.51**	0.63	**0.72**
8	0.68	**0.79**	0.68	**0.53**	0.55	**0.45**	0.63	**0.78**
9	0.72	**0.90**	0.72	**0.61**	0.61	**0.37**	0.57	**0.78**
10	0.60	**0.83**	0.60	**0.55**	0.61	**0.47**	0.62	**0.78**
11	**0.54**	0.83	**0.54**	0.54	**0.68**	0.50	**0.66**	0.73
12	**0.51**	0.76	**0.51**	0.52	**0.69**	0.44	**0.68**	0.80
13	**0.36**	0.58	**0.36**	0.39	**0.41**	0.32	**0.87**	0.86
14	**0.33**	0.63	**0.33**	0.43	**0.33**	0.23	**0.88**	0.91
15	**0.43**	0.77	**0.43**	0.53	**0.28**	0.20	**0.89**	0.87

This table presents session averages of auditors' and managers' decisions, the overall low audit effort, and the quality of audited financial statements in all treatments of all sessions. The treatments that we conducted first are shown in **bold**.

[a] We define sessions 1–15 in Table 1.

Variable Definitions:

Low Audit = frequency of the low audit effort that auditor participants chose;
Overall Low Audit = overall low audit effort, which also comprises the computerized audits;
Manipulation = frequency of manipulated financial statements manager participants chose; and
Quality = resulting quality of audited financial statements.

However, if we consider all periods, we see that there is a statistical difference between the treatments for treatment order A–B (Table 3). At the beginning, subjects in the role of auditors do not account sufficiently for the existence of computerized auditors.

Result 3: Managers manipulate financial statements less often in the presence of computerized auditors.

While standard theory predicts the same behavior of managers for both treatments, the QRE predicts a lower frequency of manipulations in the presence of computerized auditors, because managers are confronted with a higher detection probability.

The graph in the lower left-hand panel in Figure 2 clearly favors the QRE, and we also confirm H1 statistically. All tests contained in Table 3 show that the existence of computerized auditors induces managers to manipulate less frequently. This observation is particularly true when treatment B was conducted first (i.e., treatment B could not be "spoiled" by the experience of the other treatment condition).

We also apply the Wilcoxon signed-rank test to the matching group averages and present the results in Table 3. Furthermore, the probit regression in Table 4 shows that significantly fewer managers manipulate in the presence of computerized auditors, even in the final periods of the treatment. We might have expected that not only auditors, but also managers would learn, and therefore, that the difference between the treatments for the managers would also vanish over time. However, the probit regression results in Table 4 show that managers' learning is less pronounced. Therefore, the treatment difference is also significant in later periods.

The difference in learning between auditors and managers might have two explanations. First, auditors perceive the existence of computerized auditors more explicitly than do managers, because some of the auditors' decisions are automatically overridden. Managers see only the resulting audit effort, so it is more difficult for them to incorporate the presence of computerized auditors into the game. Second, the existence of computerized auditors represents a risk for the managers in the sense of lower payoffs. Therefore, computerized auditors are much more salient for the managers than for the auditors.

Result 4: The quality of audited financial statements is higher in the presence of computerized auditors.

We have shown that the introduction of computerized auditors improves the overall audit effort that is actually performed. Managers seem to anticipate the auditors' behavior and manipulate less often in the presence of these auditors, so the quality of audited financial statements increases (see the lower right-hand graph in Figure 2).

The introduction of computerized auditors improves the quality of audited financial statements, independent of the measure we look at. Table 3 shows that this finding is true for matching group averages. Table 5 confirms this result with regressions in which we use individual data with matching groups as the independent variable. Therefore, in contrast to the standard prediction, the existence of computerized auditors clearly has a positive impact on the quality of audited financial statements.

Result 5: Decision errors are more important for participants' behavior than risk aversion.

We see that the QRE makes adequate qualitative predictions. If the estimation for the error parameter would take on the same value across different experiments, we could calculate precise point predictions based on the QRE. Nevertheless, estimated values for μ

(McKelvey and Palfrey 1995) differ across experiments for the following reasons. First, since we measure the error parameter μ in monetary units or in points, we need to know the exchange rate between points and money. Second, μ depends on the complexity of the game as perceived by the participants. As related experiments differ from ours in design and framing, a comparison of μ does not seem to make sense. Therefore, we assume that μ is constant across the treatments of our experiment. This assumption makes quantitative predictions of comparative static effects possible.

The result of a maximum likelihood estimation of the pure QRE model (as described in the appendix) is $\mu = 1.11$. We show the results of the experiment together with the estimated Quantal Responses in Figure 3, which also contains the results of the robustness checks presented below.

We also estimated a model in which we combine the QRE with constant relative risk aversion. We find that no positive parameter of risk aversion can improve the estimate of the pure QRE model, which assumes that the players are risk neutral. A similar quality of the estimation of the pure QRE model can also be attained with some positive risk aversion and a smaller value of μ. This finding indicates that we cannot claim that our data contradict the levels of risk aversion reported in the literature (Goeree et al. 2000). However, these estimates do confirm that the endogenous error parameter μ, rather than risk preferences, are the principal drivers of the participants' behavior. This result is in line with Rabin (2000), who shows that risk aversion and maximizing expected utility based on total wealth are incompatible for the stake sizes usually used in experiments.

Result 6: The QRE outperforms the NE in predictive power even in static settings.

As Figure 3 shows, the predictive power of the QRE is clearly superior to that of the NE, even if we look at both treatments separately. This finding is not surprising, since the QRE has a free parameter to adjust.

However, we can make two predictions on the differences between the behavior determined by the QRE and the NE. For both treatments, the QRE predicts a smaller manipulation probability than does the NE, and a higher probability of a low audit effort for treatment A without computerized auditors. (We have no hypothesis for the auditor decision in the treatment with computerized auditors.) Binomial tests for the variable "sign of the difference between the behavior in a matching group and Nash prediction" shows a significant difference ($p < 0.05$, one-sided test, both sequences used) for both hypotheses. Even in treatment A, managers manipulated less frequently and auditors chose a low audit effort more frequently than predicted by standard theory, so that data are closer to the symmetric outcome (see Table 6). This result is statistically significant even for the last ten periods.

Robustness Checks

We performed two further experiments to show that our results do not depend on the framing or the payoff matrix of the basic experiment.

We used audit terminology in the instructions to ease understanding of the incentives. In principle, subjects might have preferred a certain action simply because of its more attractive label. For instance, auditors could have chosen a high audit effort because they thought it was socially more acceptable. Although the framing should have no influence on the predicted treatment effects, it is possible that the introduction of automatically honest auditors put even more emphasis on the social valuation of high audit effort and thus created an interaction effect between treatment and framing.

FIGURE 3
Nash Best Replies, Quantal Best Replies (Error Parameter $\mu = 1.11$), Corresponding Equilibria, and Experimental Data

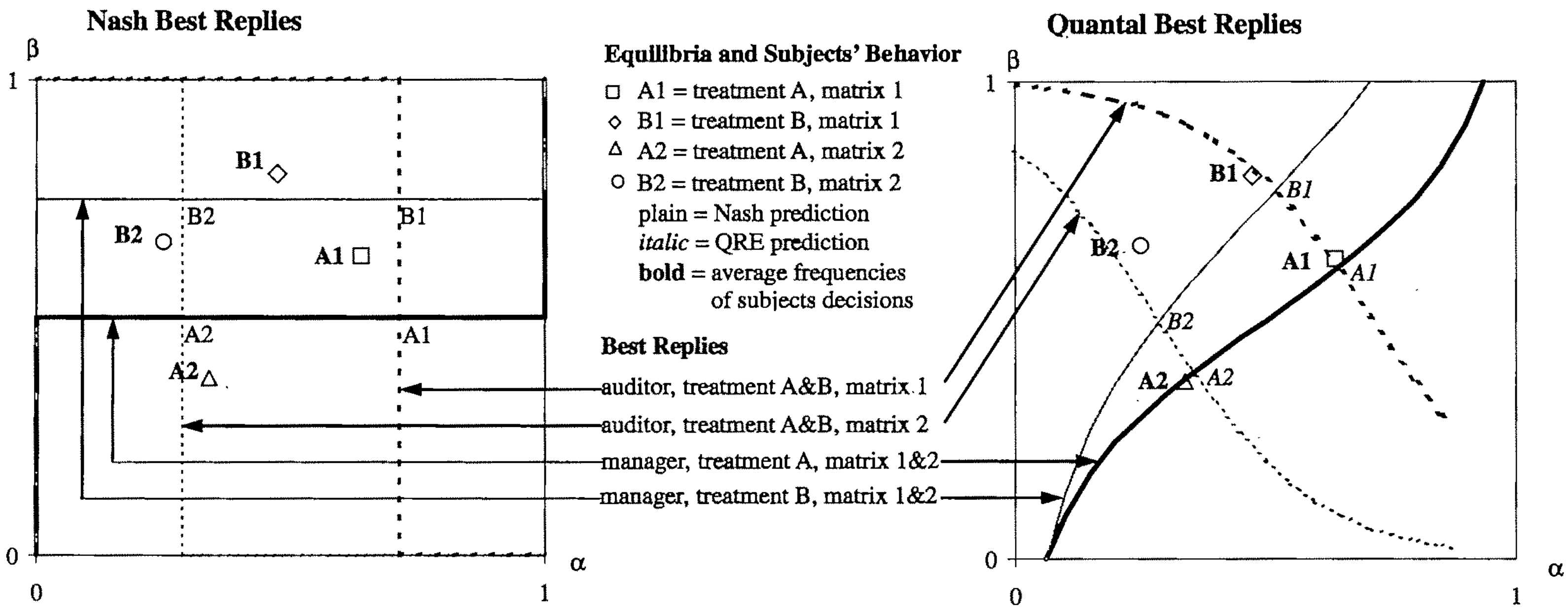


TABLE 6
Standard Predictions and Observed Behavior

		Observed Behavior[a]		
	Nash Equilibrium	**All Periods**	**Periods 1–10**	**Periods 11–20**
Treatment A (without Computerized Auditors)				
Low Audit	0.50	0.630	0.662	0.597
Overall Low Audit	0.50	0.630	0.662	0.597
Manipulation	0.714	0.638	0.631	0.646
Quality	0.643	0.601	0.582	0.619
Treatment B (with Computerized Auditors)				
Low Audit	0.75	0.802	0.801	0.803
Overall Low Audit	0.50	0.540	0.536	0.544
Manipulation	0.714	0.474	0.456	0.492
Quality	0.643	0.750	0.762	0.739

We report the prediction of the Nash Equilibrium and the average frequencies of behavior we observed in our experiment. To assess the stability of the deviations from the Nash Equilibrium, we also report the data for the first and second half of the experiment separately.

Variable Definitions:

Low Audit = frequency of the low audit effort that auditor participants chose;
Overall Low Audit = overall low audit effort, which also comprises the computerized audits;
Manipulation = frequency of manipulated financial statements manager participants chose; and
Quality = resulting quality of audited financial statements.

To exclude this possibility, we conducted two further sessions with 32 subjects each, and used a strictly neutral framing as a control treatment. Instead of "manager" and "auditor," we used the terminology "participant A" and "participant B," and labeled the actions as X, Y, I, and II. As Table 5 shows, we find that the main prediction of the QRE differs from standard prediction, i.e., managers manipulate less in treatment B compared to treatment A. Indeed, Table 7 shows that managers' average manipulation probability does not differ significantly between the two framing conditions. However, in treatment A, auditors chose a low audit effort less frequently in the neutral treatment compared to the non-neutral framing. This result is the opposite of what we would expect from a social desirability argument. Finally, in treatment B, auditors' behavior was not significantly different in the two framing conditions.

A second robustness check concerns the payoffs of the game. In the basic experiment, we implemented payoffs in line with the matching pennies games presented in the literature as a simplified model of the strategic auditor-manager interaction. Nevertheless, with payoff matrix 1, the auditor's lowest payoff is realized if he chooses a high audit effort and the financial statements are correct. As Figure 1 shows, payoff matrix 1 creates a strong effect of errors in the sense of the QRE, because the Quantal best reply of the manager differs strongly between the treatments. In a further control experiment, we used payoff matrix 2 (see Table 2), which also corresponds to a matching pennies game. Payoff matrix 2 is closer to real-world incentives, because the auditor receives his worst payoff if he fails to detect manipulations. Ninety-four students participated in the three sessions with payoff matrix 2 and the auditor-manager framing.

TABLE 7
Statistical Test of the Robustness Checks

	Low Audit	*Overall Low Audit*	*Manipulation*	*Quality*
Robustness Check 1: Neutral Framing				
Wilcoxon signed rank test (two-sided) that the variable is higher in the presence of computerized auditors in the sessions with neutral framing	2.240**	0.070	−2.457**	2.524**
Mann Whitney test (two-sided) that the variable is higher in the treatment with neutral framing (order A–B; treatment A)	−2.191**	−2.191**	0.766	1.495
Mann Whitney test (two-sided) that the variable is higher in the treatment with neutral framing (order A–B; treatment B)	0.218	0.182	−0.653	0.402
Robustness Check 2: Payoff Matrix 2				
Wilcoxon signed rank test (two-sided) that the variable is higher in the presence of computerized auditors in the sessions with payoff matrix 2	3.061***	2.638***	−2.475**	0.825
Mann Whitney test (two-sided) that the variable is higher with payoff matrix 2 (order A–B; treatment A)	−4.227***	−4.227***	−4.228***	4.247***
Mann Whitney test (two-sided) that the variable is higher with payoff matrix 2 (order A–B; treatment B)	−1.337	−1.227	−3.322***	3.179***

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*, **, *** Indicate statistical significance at 10 percent, 5 percent, and 1 percent, respectively.

This table presents statistical tests for the control treatments. Row 1 shows that the effect of computerized auditors in the treatment with neutral framing corresponds to the effects observed in the treatment with auditor-manager framing. We report the difference between the neutral framing and the auditor-manager framing in rows 2 and 3. It shows that the framing causes only one significant difference: auditors in treatment A are more likely to perform a low audit effort in the neutral framing. Row 4 shows that subjects respond equally to the introduction of computerized auditors as for payoff matrix 1 if we implement payoff matrix 2. Rows 5 and 6 show that our data replicate the Own Payoff Effect.

Variable Definitions:

Low Audit = frequency of low audit effort that auditor participants chose;
Overall Low Audit = overall low audit effort, which also comprises the computerized audits;
Manipulation = frequency of manipulated financial statements manager participants chose; and
Quality = resulting quality of audited financial statements.

Figure 3 shows the Quantal best replies for payoff matrix 2. Since we changed the auditor's payoff, only this (Quantal) best reply changes for payoff matrix 2. The figure shows that the basic predictions remain the same as for payoff matrix 1. Again, there are predictions of the QRE that differ from standard predictions: managers manipulate less if there are computerized auditors, and auditors exert more low effort audits. Since we did not change the manager's payoffs, we were also able to test whether the Own Payoff Effect was present. Whereas the NE predicts no difference in auditors' behavior between the games, the QRE predicts less low effort audits with payoff matrix 2 (for both treatments). Tables 5 and 7 confirm both hypotheses. First, as with payoff matrix 1, managers manipulate less when computerized auditors are introduced. Second, auditors exert fewer low effort audits with payoff matrix 2 than with payoff matrix 1. Although this difference is also significant in the treatment without computerized auditors, it is not significant in the treatment with computerized auditors, but with the right sign.

The robustness checks indicate that neither the framing nor a particular payoff structure is responsible for the success of the QRE. Our results indicate that errors in the sense of the QRE should be taken into account when researchers analyze auditor-manager interactions.

VI. SUMMARY AND CONCLUSION

The auditing literature studies the interrelation between a manager's reporting behavior and an auditor's effort choice on the basis of simple bimatrix games. To investigate whether observed behavior of subjects in the role of managers and auditors fits the model's standard predictions, we have conducted a laboratory experiment framed in the auditing context. In particular, we investigate the effects of varying proportions of reputable auditors on audit quality (and, indirectly, the quality of audited financial statements). In our experiment, we analyze the effect some automatically honest auditors (treatment B) have on managers' reporting behavior and on auditors' effort choices, compared to a situation in which only opportunistic auditors are present (treatment A).

In line with Nash predictions, we find that auditor subjects exert low audit effort more often if computerized auditors are present, meaning that such auditors in fact crowd out auditors who strategically choose a high audit effort. Nevertheless, and contrary to Nash predictions, this crowding-out is incomplete. Therefore, subjects in the role of managers are actually confronted with a higher probability for misstatements to be detected in treatment B. This increased detection probability causes managers to manipulate financial statements less often when they are faced with a certain percentage of computerized auditors. Taken together, this behavior leads to a higher quality of audited financial statements in the presence of computerized auditors. This observation clearly contrasts to the Nash prediction.

Our results have practical implications if there are, in reality, auditors who have individual preferences for honesty, i.e., who are conditionally trustworthy. In this case, stressing a social norm by emphasizing professional ethics might increase the proportion of auditors exerting high audit effort, even if their monetary incentives remained unchanged. Our experiment shows that—contrary to the Nash prediction—this increase in honest auditors, in turn, has a positive impact on the quality of audited financial reports.

Our results can be explained by applying the Quantal Response Equilibrium (McKelvey and Palfrey 1995), which assumes that subjects make errors and know that other subjects make errors as well. Even though the differences between observed behavior and standard theory decrease over time, the main qualitative effects are persistent. However, even if standard theory can make accurate predictions of behavior in the long run, a theory that can account for the behavior after a change in the audit environment is of value.

Finally, we find that bounded rationality is an important factor in explaining the behavior actually observed in auditor-manager settings. However, subjects' risk aversion does not play a crucial role. Neither the auditor-manager framing we used in the instructions nor the specific payoff structure we implemented account for our main results.

APPENDIX

A Formal Presentation of the Quantal Response Equilibrium

The Quantal Response Equilibrium (QRE) (Goeree et al. 2000) is based on the idea that players make errors. We present the QRE for the special case in which the error is extreme value distributed (Logit Equilibrium) for finite games with two players, A and B.

Let us fix player B's strategy s^B. Let $\pi_i^A(s^B)$ denote player A's expected payoff if he chooses action i from the set of feasible actions $S^A = \{1,\dots,a\}$. Player A's Quantal best reply is the strategy $s^A = (p_1^A, p_2^A, \dots, p_a^A)$, which we calculate according to:

$$p_i^A = \frac{\exp(\pi_i^A(s^B(/\mu)}{\sum_{j \in S^A} \exp(\pi_j^A(s^B)/\mu)}. \tag{5}$$

The parameter μ is the error parameter. It determines how sensitive choice probabilities are to payoff-differences. If the error parameter is very large, the term π_i^A/μ is close to zero. Therefore, the numerator in Equation (5) is close to 1, almost regardless of the expected payoff, and the choice probabilities are nearly the same for all actions. If $\pi_i^A > \pi_j^A$ and μ goes to zero, then the ratio between the two choice probabilities p_j^A and p_i^A, $\exp((\pi_j^A - \pi_i^A)/\mu)$, approaches zero. Therefore, the probability distribution is concentrated in the best reply for small values of the error parameter μ. A pair of strategies (s^A, s^B) is a QRE if both strategies are mutual Quantal best replies. Therefore, players not only make errors, but also they take other players' errors into account.

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THE ACCOUNTING REVIEW
Vol. 82, No. 3
2007
pp. 705–729

Information Asymmetry, Diversification, and Cost of Capital

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ABSTRACT: Recently, there has been considerable interest among accounting researchers in the relation between asymmetric information and cost of capital. A number of empirical studies document associations between risk premiums and proxies for asymmetric information such as earnings quality. However, the theoretical foundation for these studies has yet to be fully established. In this study, we consider the effects of private signals that are informative of both systematic factors and idiosyncratic shocks affecting asset payoffs in a competitive, noisy, rational expectations setting. Taking a large economy limit, we show that (1) risk premiums equal products of betas and factor risk premiums, irrespective of information asymmetries; (2) holding total information constant, greater information asymmetry leads to higher factor risk premiums and, thus, higher costs of capital; and (3) controlling for betas, there is no cross-sectional effect of information asymmetries on cost of capital. These results provide guidance in interpreting the findings of existing empirical work and suggest specifications helpful for future research.

I. INTRODUCTION

A firm's cost of capital (expected return) is the sum of the risk-free return and a risk premium. In neoclassical theory, it is widely held that risk premiums are completely determined by exposure to systematic risk or, more precisely, the product of betas and risk premiums on systematic risk factors. Idiosyncratic risks are not priced because in

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We are grateful for comments from Henry Cao, Bruce Lehman, Avanidhar Subrahmanyam, Jerry Feltham, and seminar participants at: Carnegie Mellon University; Hong Kong University; Hong Kong University of Science & Technology; Institute of Mathematics and Application at the University of Minnesota; Massachusetts Institute of Technology's Sloan School of Management; National Singapore University; Peking University; Rady School of Management at the University of California, San Diego; Singapore Management University; TsingHua University; the University of Electronic Science and Technology of China; University of Michigan's Ross Business School; University of Southern California's Marshall School of Business; University of British Columbia; University of Calgary; University of Vienna; The Pennsylvania State University; and University of North Carolina. Jing Lui is grateful for research support from the Chinese Business Research Institute, CKGSB.

Editor's note: This paper was accepted by Madhav Rajan.

Submitted June 2005
Accepted September 2006

large economies they can be eliminated by forming well-diversified portfolios. Interesting questions arise as to how asymmetric information affects a firm's cost of capital. First, how, if at all, are firm betas affected by asymmetric information? Second, how does asymmetric information affect factor risk premiums? Third, how do characteristics of the firm's information environment otherwise enter into the determination of its cost of capital? These questions are important for accounting because of its central role in reducing information asymmetry in capital markets and the presumption that the cost of capital is reduced as a consequence of accounting disclosure.[1]

In this paper, we examine the interplay between asymmetric information and cost of capital while fully considering the forces of diversification. We conduct our analysis within the framework of a competitive noisy rational expectations economy pioneered by Grossman and Stiglitz (1980) and extended by, among others, Admati (1985) and Easley and O'Hara (2004). In order to examine the effect of diversification, we follow the standard practice in the asset pricing literature (e.g., Ross 1976) and employ a factor structure to distinguish systematic and idiosyncratic components of asset payoffs. Private signals for each asset have components that are informative about systematic factors as well as idiosyncratic shocks of the underlying payoffs. Informed investors receive these signals and uninformed investors draw inferences about the information contained in these signals from prices. Our principal focus lies with characterizing risk premiums for large economies in which the number of risky assets and related private signals go to infinity.

We show that, for large economies, while private information about systematic factors affects (market-wide) factor risk premiums, it has no effect on firm betas. Private information about idiosyncratic shocks enters the determination of factor risk premiums only as a source of noise in drawing inferences about systematic factors either directly from private signals or indirectly via price. We further show that holding total information constant, factor risk premiums increase as information asymmetry about systematic factors increases. However, controlling for betas, there is no cost of capital effect in the cross-section.[2]

Our results are intuitive. Private information about systematic factors affects risk premiums by resolving uncertainty about systematic factors representing risks that are priced in large as well as small economies. Private information about idiosyncratic shocks changes expectations of future risky asset payoffs, but in large economies idiosyncratic risks are not priced. *Ceteris paribus*, greater disparity in posterior beliefs about systematic factors between informed and uninformed investors implies a higher cost of capital consistent with less resolution of systematic uncertainty. Since private signals are more informative about systematic factors than are prices, increasing the fraction of informed investors results in a greater resolution of uncertainty about those factors. Greater sensitivity of private signals to systematic factors, higher precision, and lower volatility of idiosyncratic shocks imply more is learned from those signals about systematic factors, thereby reducing risk premiums.

The above results are obtained in the space of price and dollar payoffs; this domain is consistent with the exponential utility and normal distributions structure we employ for analytic tractability. In the space of returns and payoffs scaled by price, we show that, in addition to the effect of private information on factor risk premiums, firms' costs of capital vary cross-sectionally because the price-deflated firm betas are affected by idiosyncratic factors as well as information on idiosyncratic factors. However, after controlling for deflator effects, firm-specific information characteristics have no influence on expected returns.

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[1] For example, the former SEC chairman Authur Levitt suggested in a speech at the Inter-American Development Bank on September 29, 1997, that high-quality accounting standards "improve liquidity and reduce capital costs."

[2] Looking at the extreme cases, if private signals only pertain to idiosyncratic shocks, then in large economies information leads to no resolution of uncertainty about priced risks, which implies risk premiums are unaffected. At the other extreme, if private signals are simply risky asset payoffs plus noise; then in the limit, as the economy expands, factor realizations become perfectly revealed to all investors, which implies risk premiums equal zero.

An important feature of our information structure is that private signals are informative about systematic factors as well as idiosyncratic shocks. This feature is necessary for information asymmetry to affect the risk premium and is consistent with evidence from Seyhun (1992) and Lakonishok and Lee (2001) that corporate insiders are able to time the market. Furthermore, the notion that private signals at the firm level may contain a systematic component is supported by the observation that financial reports for which some investors may have advance knowledge typically include fundamentals such as revenues, earnings, and cash flows that are plausibly affected by systematic factors as well as idiosyncratic shocks; evidence dates back to Ball and Brown (1968).

Although private signals at the firm-level are generally understood to be far more informative of idiosyncratic shocks than systematic factors, we will show that an infinitesimally small amount of information on systematic factors extracted from private signals for each firm, when aggregated in large economies, can have a finite effect on factor risk premiums.

Our model can be viewed as an extension of Easley and O'Hara (2004) to a setting where both private signals and asset payoffs follow a factor structure. Like us, Admati (1985) also considers the interplay between private information and equilibrium prices in a noisy rational expectations framework.[3] Rather than a factor structure, Admati's (1985) principal analysis assumes asset payoffs are distributed normally and satisfy a general variance-covariance matrix. However, Admati (1982) recognizes the advantages of a factor structure in characterizing economy-wide information, but finds an explicit solution in the case of diverse information to be infeasible.

In contrast to Admati (1985), our asymmetric information structure allows us to solve for equilibrium prices and risk premiums explicitly. Having an explicit pricing solution is especially useful because it allows us to examine how changes in model parameters such as posterior information asymmetry affect factor risk premiums. Furthermore, having a closed-form solution for finite economies enables us to examine the convergence properties of risk premiums as the number of assets increases. Apart from the factor structure, our analysis departs from Easley and O'Hara (2004) by examining large as well as small economies, while they consider only finite economies where the full force of diversification is not present.

We note that outside of the paradigm that we employ to study asymmetric information and cost of capital, there is another stream of literature that examines the effect of estimation risk on cost of capital (e.g., Barry and Brown 1985; Handa and Linn 1993; Coles et al. 1995; Clarkson et al. 1996). However, as Clarkson and Thompson (1990) observe, in theory, estimation risk is fully diversifiable in large economies.

This study has a number of important implications for the voluminous empirical literature that examines the link between information asymmetry and cost of capital. First, our analysis clarifies a commonly held misperception about the pricing of asymmetric information in a competitive market (e.g., Easley and O'Hara 2004). A large number of empirical studies cite the flawed intuition that information asymmetry should be priced because uninformed investors demand price protection from trading with privately informed investors.[4] However, this price-protection intuition is not really supported by Easley and O'Hara's (2004) analysis because in fact both informed and uninformed investors exploit liquidity traders whose demands are manifested in an assumption of noisy supply. Our analysis demonstrates that the pricing effect characterized in Easley and O'Hara's (2004) Proposition 2 is driven by under-diversification, and will disappear when the economy is large.

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[3] Brennan and Cao (1997) employ a similar structure. Admati (1985) is a multi-asset generalization of Hellwig (1980).

Second, we show that factor risk premiums are affected by information asymmetry. In particular, controlling for total information, high (low) information asymmetry leads to high (low) cost of capital. This result provides theoretical support for empirical studies that examine the link between the aggregate information environment and the market cost of capital (see, for example, Bhattacharya et al. 2003; Bhattacharya and Daouk 2002; Jain 2005).

Third, our analysis suggests that firm-specific information characteristics should not enter the determination of expected returns after controlling for beta. Empirical research in this domain reports conflicting evidence. While some claim to have found a positive correlation between information asymmetry and cost of capital (e.g., Botosan 1997; Botosan and Plumlee 2002; Botosan et al. 2004), others argue that endogeneity and correlated omitted variables are serious confounding factors (e.g., Cohen 2004; Nikolaev and van Lent 2005; Chen et al. 2004). Our theory sheds useful light on this issue (see Section IV for details).

Fourth, our analysis suggests that information asymmetry has no effect on cash flow betas, but may alter (percentage) return betas due to deflation by price. Existing empirical studies do not investigate the information effect on cash flow betas or contrast the difference between cash flow and return betas. Hence, our theory suggests that this is a promising arena for further inquiry.

Finally, an asymmetric information factor does not arise endogenously in our model. However, our model is silent on whether there exists a systematic information factor. Therefore, our model is not inconsistent with studies that assume the existence of an information factor (e.g., Aboody et al. 2005; Francis et al. 2004b; Easley et al. 2002). We believe both theoretical and empirical research on this issue is warranted.

The rest of the paper is organized as follows: Section II describes the setup for our model and studies an economy with a small number of investors and risky assets; Section III studies the large economy limit; Section IV explores the empirical implications of the model; and Section V concludes the paper.

II. SMALL ECONOMY

In this section, we consider an economy with a finite number of risky assets. We present a noisy rational expectation model in which the asset payoffs and the random supply of the assets have factor structures. We solve the equilibrium in closed-form and consider special cases that serve as a benchmark when information is symmetric.

The Setup

We assume that there is a riskless asset with return R_f that has an infinitely elastic supply. We assume that payoffs of N risky assets are generated by a factor structure of the form:

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[4] See, for example, Ali et al. (2005), Botosan and Plumlee (2002), Botosan et al. (2004), Bushman et al. (2005), Easley et al. (2002), Hope et al. (2005), Francis et al. (2004a), Gietzmann and Ireland (2005), Gore and Baber (2005), and Bhattacharya et al. (2003).

$$\upsilon = \bar{\upsilon} + \beta F + \Sigma^{1/2}\varepsilon, \tag{1}$$

where $\bar{\upsilon}$ is a $N \times 1$ constant vector, the factor F is a $K \times 1$ vector of mean zero normal random variables with covariance matrix Σ_F, the factor loading β is a $N \times K$ constant matrix, the idiosyncratic risk ε is a vector of standard normal random variables, and Σ is an $N \times N$ diagonal matrix.

The supply of risky assets, x, is a vector of $N \times 1$ random variables specified as follows:

$$x = \bar{x} + \beta_x F_x + \Sigma_x^{1/2}\eta_x \tag{2}$$

where x, β_x, and Σ_x are constant $N \times 1$ vector, and $N \times N$ matrix, respectively. There is a systematic component in the random supply, F_x, which is a mean zero normal random variable with variance σ_{fx}^2 and an idiosyncratic component, η_x, which is a standard normal random variable.

The noisiness of the supply is necessary in our setting to prevent prices from fully revealing the informed investors' private signal (defined below) and can be interpreted as caused by trading for liquidity reasons. The presence of a systematic component is based on the reasonable view that liquidity trading is influenced by market-wide forces that may or may not correspond to factors influencing risky asset payoffs.

If we interpret the random supply as due to a liquidity effect, then our assumption of systematic components in random supply is supported by empirical studies that find there are systematic components of liquidity; for example, Chordia et al. (2000) and Huberman and Hulka (2001). IPO waves are also suggestive of systematic components. Without a systematic component in the random supply, then in the limiting case, as the number of risky assets becomes large (implying an infinite number of independent asset specific signals), prices would still be fully revealing of the informed investors' private signals. In other words, noisy supply is necessary but not sufficient to ensure that asymmetric information is not a moot issue in large economies; there also needs to be a systematic component. We further assume for simplicity that F_x is independent of the factors generating asset payoffs.[5]

We assume that there are two classes of investors, informed and uninformed, with the total number denoted by M. In the finite economy, the number of investors can be independent of the number of assets. In the large economy limit, M and N must expand at the same rate to ensure that per capita wealth is neither zero nor infinite (see Section III for details). Investors are assumed to be price takers, though strictly speaking this is only suitable in the limiting case where the numbers of assets and investors are infinite.

The informed investors all receive private signal s on asset payoffs and the uninformed can only (imperfectly) infer the signal from market prices. This specification is used by Grossman and Stiglitz (1980) and Easley and O'Hara (2004). In Admati (1985) agents receive independent signals. It can be argued that our assumption and Admati's (1985) are two special cases of a general information structure where investors have both diverse and asymmetric information; while we emphasize asymmetry, Admati (1985) emphasizes diversity. Technically speaking, the correlation between the private signals across informed investors is perfect in our model and zero in Admati's (1985) model. While in our analysis price will be a function of informed investors' private information, price is a function of the realized asset payoffs in Admati's (1985) case when the number of assets is infinite due to the elimination of signal noise through aggregation of signals across assets.

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[5] Noisy rational expectation equilibrium models with many assets having a factor structure in asset payoffs, but not in the random supply of risky assets, have been considered in Caballe and Krishnan (1994), Daniel et al. (2001), Kodres and Pritsker (2002), and Pasquariello (2004).

We assume all investors have the following utility function:

$$U = -\mathrm{E}[\exp(-AW_1)], \tag{3}$$

where A is the investor's absolute risk aversion coefficient and W_1 is the investor's terminal wealth. The budget constraint is:

$$W_1 = W_0R_f + D(\upsilon - R_fp), \tag{4}$$

where W_0 is the investor's initial wealth, p is the vector of security prices, and D is a vector containing the numbers of shares invested in risky assets.

The normal distribution assumption for asset payoffs implies that the utility maximization problem can be transformed into a mean-variance optimization problem:

$$\max_D \mathrm{E}(W_1|J) - \frac{A}{2}\mathrm{var}(W_1|J)$$

$$s.t.\ W_1 = W_0R_f + D(\upsilon - R_fp)$$

where J represents the investor's information set. The first-order condition implies optimal demand takes the following form:

$$D_J^* = \frac{W_0}{\gamma}\Sigma_{\upsilon|J}^{-1}\mathrm{E}(\upsilon - R_fp|J). \tag{5}$$

When asset payoffs do not depend on systematic factors, $\beta = 0$, it is easy to show investors' demands for securities are increasing in expected asset payoffs and the precision of information about asset payoffs, and decreasing in risk aversion. In the more general case where asset payoffs do depend on systematic factors, $\beta \neq 0$, the demand for asset i depends not only on investors' posterior precision of beliefs on payoffs for asset i, but also on their posterior beliefs on payoffs for other assets. The informed and the uninformed have different demands because they condition on different information sets J.

Informed Investors

The informed investors receive private signal s, which takes the form:

$$s = (\upsilon - \bar{\upsilon} - \beta F) + bF + \Sigma_s^{1/2}\eta = \Sigma^{1/2}\varepsilon + bF + \Sigma_s^{1/2}\eta. \tag{6}$$

The $N \times K$ constant matrix b reflects the relative information content of the signal with respect to the systematic factors and η is an $N \times 1$ standard normal random variable. To conform with the interpretation of factor models, we will assume that F, ε, η, and η_x are jointly normal and independent and the matrices Σ_s and Σ_x are diagonal.

Our specification of asset payoffs is distinct from an alternative specification where asset payoffs do not follow a factor structure, but satisfy a general variance-covariance matrix (e.g., Admati 1985). Though a factor structure such as (1) implies a specific variance-covariance matrix, a general variance-covariance matrix does not imply a corresponding factor structure. Admati (1985) entertains such constructions and concludes that a factor model is the natural context in which to consider private signals on economy-wide phenomena. Under her information structure, investors receive private signals about both systematic factors and idiosyncratic shocks. However, because of mathematical complexities an explicit solution was not obtained.

The signal s for each risky asset specified in the above equation is a linear combination of information about the systematic components of the asset's payoff, information about the idiosyncratic component of that payoff, and noise. The signal s can also be interpreted as a combination of two signals: a signal about the idiosyncratic component of asset payoffs, $s_1 = \Sigma^{1/2}\varepsilon + (bF + \Sigma_s^{1/2}\eta)$, where $(bF + \Sigma_s^{1/2}\eta)$ is interpreted as noise; and a signal about the systematic component, $s_2 = bF + (\Sigma^{1/2}\varepsilon + + \Sigma_s^{1/2}\eta)$, where $(\Sigma^{1/2}\varepsilon + + \Sigma_s^{1/2}\eta)$ is interpreted as noise.

The assumption that informed investors receive private information not only about the idiosyncratic component, but also about the systematic components of risky asset payoffs, although uncommon in the theoretical literature, is intuitive. Informed investors such as corporate insiders are likely to know more than the general public about the firm's fundamentals such as revenues, earnings, and cash flows. To the extent that the fundamentals are generated by a factor structure, private information is likely to contain both components. Consistent with this assumption, Seyhun (1992) and Lakonishok and Lee (2001) show that aggregated trading by corporate insiders is predictive of future market returns.

Our specification of signals differs in two respects from that of Admati (1982) in the context of her factor model: the signals in our model are perfectly correlated across informed investors, while in her model investors receive diverse signals, and the "two signals" constructively received by informed investors in our model are correlated with covariance matrix Σ_s conditional on v and F, whereas the two signals for a given investor in Admati (1982) are uncorrelated. While the former difference is important because it enables us to obtain a closed-form solution under a factor structure of asset payoffs, the latter difference does not affect either the structure of the explicit solution or the qualitative results that follow from that solution.

To calculate the conditional expectations and covariance matrixes, we note that the joint density function of v, F, and s implies the following moment conditions:

Remark 1:

$$E(\upsilon|s,F) = \bar{\upsilon} + \beta F + \Sigma_{\upsilon|s,F}\Sigma_s^{-1}(s - bF)$$

$$E(F|s) = \Sigma_{F|s}b'(\Sigma + \Sigma_s)^{-1}s$$

$$\Sigma_{\upsilon|s,F}^{-1} = \Sigma^{-1} + \Sigma_s^{-1}$$

$$\Sigma_{F|s}^{-1} = \Sigma_F^{-1} + b'(\Sigma + \Sigma_s)^{-1}b$$

$$\hat{\Sigma}_s^{-1} = \Sigma + b'\Sigma_F b + \Sigma_s.$$

All proofs are given in the Appendix.

From these moments, it follows that, conditional on s and F, the payoff v can be written in the form:

$$\upsilon = \bar{\upsilon} + \Sigma_{\upsilon|s,F}\Sigma_s^{-1}s + (\beta - \Sigma_{\upsilon|s,F}\Sigma_s^{-1}b)F + \varepsilon_{\upsilon|s,F} \tag{7}$$

where, conditional on s and F, $\varepsilon_{\upsilon|s,F}$ is an standard normal random variable. We note that from the perspective of an informed investor loadings on the systematic factors (conditional betas) are $\beta_s = \beta - \Sigma_{\upsilon|s,F}\Sigma_s^{-1}b$. The precision matrix of the factors has increased from Σ_F^{-1} to $b'(\Sigma + \Sigma_s)^{-1}b$. Note idiosyncratic risks matter for the factor precision matrix in that they are a source of noise when informed investors draw inferences about systematic factors.

From Equation (7), the expectation of υ conditional on s is:

$$E(\upsilon|s) = \bar{\upsilon} + \Sigma_{\upsilon|s,F}\Sigma_s^{-1}s + (\beta - \Sigma_{\upsilon|s,F}\Sigma_s^{-1}b)\Sigma_{F|s}b'(\Sigma + \Sigma_s)^{-1}s \tag{8}$$

and the variance of υ conditional on s is:

$$\Sigma_{\upsilon|s} = \Sigma_{\upsilon|s,F} + (\beta - \Sigma_{\upsilon|s,F}\Sigma_s^{-1}b)\Sigma_{F|s}(\beta - \Sigma_{\upsilon|s,F}\Sigma_s^{-1}b)'. \tag{9}$$

Equations (8) and (9) can be substituted into the demand function to calculate the investor's demand D_s for risky assets:

$$D_s^* = \frac{1}{A}\Sigma_{\upsilon|s}^{-1}(\bar{\upsilon} + \Phi_s s - R_f p) \tag{10}$$

where $\Phi_s = \Sigma_{\upsilon|F,s}\Sigma_s^{-1} - (\beta + \Sigma_{\upsilon|F,s}\Sigma_s^{-1}b)\Sigma_{F|s}b'(\Sigma + \Sigma_s)^{-1}$.

Uninformed Investors

The uninformed investors do not observe the signal s, but can imperfectly infer s from the equilibrium price. We conjecture that the equilibrium prices have the following form:

$$p = C + B(s - \lambda(x - \bar{x})),$$

where C is an $N \times 1$ vector and B and λ are $N \times N$ matrices. We will assume that B is invertible. Therefore, observing the price p is equivalent to observing θ, which is defined as:

$$\theta = B^{-1}(p - C) = s - \lambda(x - \bar{x}).$$

Substituting Equations (2) and (6), we can write:

$$\theta = \upsilon - \bar{\upsilon} - \beta F + bF + \Sigma_s^{1/2}\eta - \lambda\beta_x F_x - \lambda\Sigma_x^{1/2}\eta_x. \tag{11}$$

Therefore, we can interpret θ as another signal that has sensitivity b to the factor F and idiosyncratic shocks with covariance matrix Σ_θ, where:

$$\Sigma_\theta = \Sigma_s + \lambda(\beta_x\Sigma_{Fx}\beta_x' + \Sigma_x)\lambda'.$$

Note that signal θ is less informative than signal s, i.e., its conditional variance-covariance matrix is larger than that of s, i.e., $\Sigma_\theta = \Sigma_s + \lambda(\beta_x\Sigma_{Fx}\beta_x' + \Sigma_x)\lambda' \geq \Sigma_s$. We should remark that λ is in general nondiagonal; the idiosyncratic shocks $\Sigma_s^{1/2}\eta + \lambda\Sigma_x^{1/2}\eta_x$, although independent of F, are not independent of each other.

When systematic factors in the random supply are uncorrelated with systematic factors in asset payoffs, as we assumed, the signal s is a suffcient statistic for (s, θ). However, it is plausible that the two systematic factors are correlated. In this case, the signal s is no longer a sufficient statistic for (s, θ). While the uninformed will continue to condition on only θ, the informed will now condition on both s and θ, a departure from the above analysis in which the informed only conditioned on s. We assume independence for tractability. Nonetheless, we are confident that our analysis can be extended to accommodate the case of correlated factors and that our results are robust with respect to the relaxation of the independence assumption. The crucial aspect for risk premiums to be affected by asymmetric information is whether the informed investors learn more about systematic factors that influence asset payoffs than uninformed investors in equilibrium; this can be modeled with or without the correlation between the two classes of systematic factors.

The joint density function of v, F, and θ implies the following moment conditions:

Remark 2:

$$E(\upsilon|\theta, F) = \bar{\upsilon} + \beta F + \Sigma_{\upsilon|\theta,F}\Sigma_\theta^{-1}(\theta - bF)$$

$$E(F|\theta) = \Sigma_{F|\theta}b'(\Sigma + \Sigma_\theta)^{-1}\theta$$

$$\Sigma_{\upsilon|\theta,F}^{-1} = \Sigma^{-1} + \Sigma_\theta^{-1}$$

$$\Sigma_{F|\theta}^{-1} = \Sigma_F^{-1} + b'(\Sigma + \Sigma_\theta)^{-1}b$$

$$\hat{\Sigma}_\theta^{-1} = \Sigma + b'\Sigma_F b + \Sigma_\theta.$$

From these moments, it follows that, conditional on s and F, the payoff υ can be written in the form:

$$\upsilon = \bar{\upsilon} + \Sigma_{\upsilon|\theta,F}\Sigma_\theta^{-1}\theta + (\beta - \Sigma_{\upsilon|\theta,F}\Sigma_\theta^{-1}b)F + \varepsilon_{\upsilon|\theta,F} \tag{12}$$

where, conditional on θ and F, $\varepsilon_{\upsilon|\theta,F}$ is an standard normal random variable. We note that from the perspective of an informed investor loadings on the systematic factors (conditional betas) are $\beta_\theta = \beta - \Sigma_{\upsilon|\theta,F}\Sigma_\theta^{-1}b$. The precision matrix of the factors has increased from Σ_F^{-} to $b'(\Sigma + \Sigma_\theta)^{-1}b$.

From Equation (12), the expectation of υ conditional on θ is:

$$E(\upsilon|\theta) = \bar{\upsilon} + \Sigma_{\upsilon|\theta,F}\Sigma_\theta^{-1}\theta + (\beta - \Sigma_{\upsilon|\theta,F}\Sigma_\theta^{-1}b)\Sigma_{F|\theta}b'(\Sigma + \Sigma_\theta)^{-1}\theta \tag{13}$$

and the variance of υ conditional on θ is:

$$\Sigma_{\upsilon|\theta} = \Sigma_{\upsilon|\theta,F} + (\beta - \Sigma_{\upsilon|\theta,F}\Sigma_\theta^{-1}b)\Sigma_{F|\theta}(\beta - \Sigma_{\upsilon|\theta,F}\Sigma_\theta^{-1}b)'. \tag{14}$$

Equations (13) and (14) can be substituted into the demand function to calculate the uninformed investor's demand D_θ for risky assets:

$$D_\theta^* = \frac{1}{A}\,\Sigma_{\upsilon|\theta}^{-1}(\bar{\upsilon} + \Phi_\theta\theta - R_f p) \tag{15}$$

where $\Phi_\theta = \Sigma_{\upsilon|F,\theta}\Sigma_\theta^{-1} - (\beta + \Sigma_{\upsilon|F,\theta}\Sigma_\theta^{-1}b)\Sigma_{F|\theta}b'(\Sigma + \Sigma_\theta)^{-1}$.

Equilibrium

Imposing the market-clearing condition that the total demand from the informed and the uninformed investors equals the supply, we obtain the following equation:

$$x = M\left(\frac{u}{A}\,\Sigma_{\upsilon|s}^{-1}(\bar{\upsilon} + \Phi_s\theta - R_f p) + \frac{1-u}{A}\,\Sigma_{\upsilon|\theta}^{-1}(\bar{\upsilon} + \Phi_\theta\theta - R_f p)\right),$$

where μ is the proportion of informed investors. Defining $\bar{\Sigma}_\upsilon = (u\Sigma_{\upsilon|s}^{-1} + (1-u)\Sigma_{\upsilon|\theta}^{-1})^{-1}$, we derive the following expression for the prices of risky assets:

$$\begin{aligned} p &= \frac{1}{R_f}\left(\bar{\upsilon} + \bar{\Sigma}_\upsilon\left(u\Sigma_{\upsilon|s}^{-1}\Phi_s s + (1-u)\Sigma_{\upsilon|\theta}^{-1}\Phi_\theta\theta - \frac{A}{M}\,x\right)\right) \\ &= \frac{1}{R_f}\left(\bar{\upsilon} - \bar{\Sigma}_\upsilon\frac{A}{M}\,\bar{x}\right) + \frac{1}{R_f}\,\bar{\Sigma}_\upsilon u\Sigma_{\upsilon|s}^{-1}\Phi_s\left(s - (u\Sigma_{\upsilon|s}^{-1}\Phi_s)^{-1}\frac{A}{M}\,(x - \bar{x})\right) \\ &\quad + \frac{1}{R_f}\,\bar{\Sigma}_\upsilon(1-u)\Sigma_{\upsilon|\theta}^{-1}\Phi_\theta(s - \lambda(x - \bar{x})). \end{aligned} \tag{16}$$

Comparing the above expression to the conjectured form of the price p, it must be true that:

$$\lambda = (u\Sigma_{\upsilon|s}^{-1}\Phi_s)^{-1}\,\frac{A}{M}. \tag{17}$$

Note that λ is solved in terms of the parameters of the model. The matrices $\Sigma_{\upsilon|\theta}$, Φ_θ, and $\bar{\Sigma}_\upsilon$ are expressed in terms of λ as well as the parameters of the model; they are solved once λ is solved.

Theorem 1: Given that informed investors receive a private signal, s, that is informative about both idiosyncratic and systematic components of asset payoffs, a partially revealing noisy rational expectations equilibrium exists, and prices of risky assets satisfy:

$$p = \frac{1}{R_f}\bar{\upsilon} - \frac{1}{R_f}\bar{\Sigma}_\upsilon\frac{A}{M}\bar{x} + \frac{1}{R_f}\bar{\Sigma}_\upsilon(u\Sigma_{\upsilon|s}^{-1}\Phi_s s + (1-u)\Sigma_{\upsilon|\theta}^{-1}\Phi_\theta\theta)(s - \lambda(x - \bar{x})). \tag{18}$$

The risk premium of assets satisfies:

$$\mathrm{E}(\bar{\upsilon} - R_f p) = \frac{A}{M}\,\bar{\Sigma}_\upsilon\bar{x} = \frac{A}{M}\,(u\Sigma_{\upsilon|s}^{-1} + (1-u)\Sigma_{\upsilon|\theta}^{-1})^{-1}\bar{x}. \tag{19}$$

Proof: The price p and the expressions for B and C are derived by combining the Equations (16) and (17). The equation for the risk premium follows immediately. Note that the posterior precisions $\Sigma_{\upsilon|s}^{-1}$ and $\Sigma_{\upsilon|\theta}^{-1}$ do not depend on realizations of signals s and θ, respectively.

The first term in the price p in Equation (18) is the expected payoff without information discounted by the risk-free return. This is the price if investors are risk-neutral ($A = 0$) and there are no signals in the economy. The second term is the discount in price associated with risk, thus the risk premium. The third term is the correction to the expected payoff associated with signals and noisy supply.

The risk premium is determined by the geometric average of the covariance matrices of asset payoffs conditional on s and θ, $\Sigma_{\upsilon|s}$, and $\Sigma_{\upsilon|\theta}$. That is, the risk premium compensates the average of the risks conditional on s and θ. Two properties of the risk premium follow. First, from Equation (9), $\Sigma_{\upsilon|s} = \Sigma_{\upsilon|s,F} + (\beta - \Sigma_{\upsilon|s,F}\Sigma_s^{-1}b)\Sigma_{F|s}(\beta - \Sigma_{\upsilon|s,F}\Sigma_s^{-1}b)'$ and similarly for $\Sigma_{\upsilon|\theta}$, the average risk includes idiosyncratic risk $\Sigma_{\upsilon|s}$ and $\Sigma_{\upsilon|\theta}$. Therefore, idiosyncratic risks are priced. Second, the average covariance matrix, $\bar{\Sigma}_{\upsilon}$ depends on β nonlinearly, thus the risk premium depends on β nonlinearly.

Symmetric Information

When all investors are informed, $\mu = 1$, Theorem 1 implies that the risk premium is:

$$E(\bar{\upsilon} - R_f p) = \frac{A}{M}\Sigma_{\upsilon|s}\bar{x} = (\Sigma_{\upsilon|s,F} + (\beta - \Sigma_{\upsilon|s,F}\Sigma_s^{-1}b)\Sigma_{F|s}(\beta - \Sigma_{\upsilon|s,F}\Sigma_s^{-1}b)')\frac{A}{M}\bar{x}.$$

In such an economy, the factor risk premiums are decreased by information because $\Sigma_{F|s}$ is smaller than Σ_F. In addition, an econometrician who observes the return but not the signal will conclude that the risk premium depends on β as well as some firm-specific characteristics, $\Sigma_{\upsilon|s,F}\Sigma_s^{-1}b$. Thus, firms with the same β but different $\Sigma_{\upsilon|s,F}\Sigma_s^{-1}b$ may have different expected returns.[6]

At the other extreme, when all investors are uninformed, $\mu = 0$, $\lambda \rightarrow \infty$, i.e., the inferred signal θ is infinitely more noisy than s and thus is not informative at all. It follows immediately that the covariance matrix conditional on θ, $\Sigma_{\upsilon|\theta}$, is the same as Σ and the factor covariance matrix conditional on θ, $\Sigma_{F|\theta}$, is the same as Σ_F. Furthermore, factor loadings conditional on θ are the same as unconditional factor loadings, i.e., $\beta_\theta = \beta$. From Theorem 1, the risk premium is:

$$E(\bar{\upsilon} - R_f p) = \frac{A}{M}\Sigma_{\upsilon|\theta}\bar{x} = (\Sigma + \beta\Sigma_F\beta')\frac{A}{M}\bar{x}. \tag{20}$$

The above can be described as the risk premium in a small economy with homogeneous beliefs. In this case, there is no updating of beliefs, idiosyncratic risk is priced, and β appears linearly in the risk premium. The idea of diversification is clearly embedded in the above expression because the risk premiums on systematic risks $(\beta\Sigma_F\beta')\frac{A}{M}\bar{x}$ are larger than the risk premium on idiosyncratic risks $\Sigma\frac{A}{M}\bar{x}$ by a factor of N. When the economy (N and M) is small, the risk premium on idiosyncratic risk can not be ignored; when the size of the economy increases, idiosyncratic risk premium becomes less and less important relative to the systematic risk premiums. As we show in the next section, in the large economy limit, idiosyncratic risk premium goes to zero.

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[6] In a recent paper, Lambert et al. (2005) also consider the effects of information disclosure on cost of capital in a general variance-covariance matrix framework. They show that if all pair-wise correlations of asset payoffs are positive, then information can reduce the average cost of capital. Our factor structure does not require a restriction to positive pair-wise correlations and allows us to distinguish effects of information (split into systematic and idiosyncratic components) on asset betas and factor risk premiums. Moreover, the principal focus of our study is on the effects of asymmetric information and price discovery, issues that are absent in Lambert et al. (2005).

We conclude this section by observing that in an economy with a finite number of assets, idiosyncratic as well as systematic risk is priced, information on idiosyncratic shocks reduces idiosyncratic risk and, hence, the risk premium, information can increase or decrease factor loadings, the risk premium depends on beta nonlinearly, and information on the systematic factor reduces systematic risk and, hence, the factor risk premium. As we will demonstrate in Section III, only the last property survives in the limit as the number of risky assets goes to infinity.

III. LARGE ECONOMY LIMIT

In this section, we study the effects of private signals on risk premiums when the economy is large in the sense that the number of assets and the number of investors go to infinity. The idea of diversification requires that the number of assets be sufficiently large for idiosyncratic shocks to cancel each other out, thereby reducing aggregate portfolio uncertainty to the point where it reflects only systematic risks. If we only expand the number of assets and fix the number of investors, however, the large economy becomes unrealistic since each individual would have infinite wealth in the limit. In addition, because each individual is assumed to have constant absolute risk aversion, infinite wealth implies infinite relative risk aversion and, hence, infinite risk premiums.[7]

To rule out such an unrealistic scenario, we require that the number of investors expand at the same rate as the number of assets so that in the $\frac{N}{M}$ approaches a constant. Without loss of generality, we assume the constant is unity. In analysis that follows, when we talk about taking the "large economy limit" or "large N limit," we are implicitly referring to the case where M and N are expanding at the same rate. Similar restrictions have been adopted in studies that concern large economies (e.g. Leuz and Verrecchia 2005; Ou-Yang 2005). In his formal derivation of the APT relation in an economy without private signals, Ross (1976) directly imposes the restriction that relative aggregate risk aversion be uniformly bounded as the number of assets and, hence, wealth increases. Our assumption satisfies his restriction. To be precise, the risk premium from the previous section for a large economy in which the numbers of both assets and investors go to infinity at the same rate when there are no private signals (i.e., homogeneous beliefs) becomes:

$$E(\bar{v} - R_f p) = \beta A\Sigma_F\beta'\bar{x}/N \tag{21}$$

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[7] We have assumed that the net supply of each individual asset is bounded below at a level above zero—stocks and bonds are such securities. If we instead let the net supply of individual assets converge to zero as the number of assets expands (e.g., increase the number of derivative securities which have net zero supply), but hold the number investors constant, wealth and risk premium will also be finite. However, in this case diversification will not be complete, even in the case of symmetric information. We rule out this case because we need diversification to work at least under symmetric information in order to study the joint effect of diversification and information asymmetry.

where $A\Sigma_F\beta'\bar{x}/N$ is a vector of factor premiums. Note that the magnitude of $\beta'\bar{x}$ is of order N and, hence, the magnitude of $\beta'\bar{x}/N$ is of order 1 when $N \rightarrow \infty$. Thus, we have finite risk premiums. In equilibrium, the risk premium on idiosyncratic risk goes to zero as $N \rightarrow \infty$; firm's risk premiums increase in betas and factor risk premiums, where the factor risk premiums in turn increase in risk aversion, aggregate uncertainty in the economy, and supply of risky assets.

We begin our analysis of the effects of private signals on risk premiums in the large economy limit with two special cases that have appeared in the literature; private information only on idiosyncratic shocks and private information on total asset payoffs. We then consider the case where private information on systematic factors has finite aggregate precision.

Special Cases

Private Information Only on Idiosyncratic Components of Asset Payoffs

Suppose informed investors receive private signals on just the idiosyncratic components of risky asset payoffs. In this case, $b = 0$ and the signals can be written as:

$$s = (\upsilon - \bar{\upsilon} - \beta F) + \Sigma_s^{1/2}\eta = \Sigma^{1/2}\varepsilon + \Sigma_s^{1/2}\eta. \quad (22)$$

Note that when $\beta = 0$, the asset payoffs are correlated. In the special case where all asset payoffs are uncorrelated, i.e., $\beta = 0$, this structure reduces to the setting considered by Easley and O'Hara (2004). It is easy to see that, for finite N, information solely about idiosyncratic shocks reduces uncertainty about priced risks. However, as we show below, in the limit as $N \rightarrow \infty$, elimination of idiosyncratic risks through diversification implies that private signals containing only idiosyncratic components have no effects on risk premiums:

Proposition 1: Given that informed investors receive private signals only about the idiosyncratic components of asset payoffs, in the limit as $N \rightarrow \infty$, the risk premium satisfies:

$$E(\bar{\upsilon} - R_f p) = \beta A\Sigma_F\beta'\bar{x}/N. \quad (23)$$

The proof is given in the Appendix.

Notably, the risk premium in this case is the same as the risk premium without information, $\mu = 0$, $\beta A\Sigma_F\beta'\bar{x}/N$, implying that this is the risk premium for all μ. In other words, there is no resolution of uncertainty about systematic factors from private signals that do not contain a systematic component; investors remain with their prior (homogeneous) beliefs.[8] It is also clear that, in the setting studied by Easley and O'Hara (2004), where $\beta = 0$, the risk premium is reduced to zero, i.e., $E(\bar{\upsilon} - R_f p) = 0$.

More generally, we expect that the same results will hold as long as $b'(\Sigma + \Sigma_s)^{-1}b \rightarrow 0$ $b'(\Sigma + \Sigma_\theta)^{-1}b \rightarrow 0$ when $N \rightarrow \infty$. Intuitively, diversification works at the power of $1/N$, implying that if the systematic component of the signal has a power less than $1/N$, then it will be eliminated by diversification.

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[8] Note further, that the proposition holds notwithstanding systematic components in the random supply, $\beta_x\sigma_{fx} \neq 0$. This result is quite intuitive: Even if all the agents are informed, $\mu = 1$, there is no resolution of uncertainty about the factors that affect asset payoffs, implying that the random supply of assets is irrelevant for asset pricing when it is independent of systematic factors.

Note that although private information on idiosyncratic shocks does not affect risk premiums in this case, it does affect asset prices and portfolio holdings of informed and uninformed investors and, hence, their expected utilities.

Private Information on Total Risky Asset Payoffs

Suppose now that informed investors receive private signals about total asset payoffs. In this case, $b = \beta$ and the signals can be written as:

$$s = (\upsilon - \bar{\upsilon}) + \Sigma_s^{1/2}\eta. \tag{24}$$

This is a special case of Admati (1985) where the covariance matrix of the assets has the form of a factor structure and the signals for different assets are uncorrelated.

In this case, $\Sigma_{F|s}^{-1} = \Sigma_F^{-1} + \beta'(\Sigma + \Sigma_s)^{-1}\beta$, which goes to infinity as $N \rightarrow \infty$. Therefore, we have:

$$\Sigma_{F|s} = 0.$$

Similarly, $\Sigma_{F|\theta}^{-1} = \Sigma_F^{-1} + \beta'(\Sigma + \Sigma_\theta)^{-1}\beta$, which also goes to infinity as long as $\beta + \beta_x\beta + \beta x$ goes to a constant as $N \rightarrow \infty$; thus we also have:

$$\Sigma_{F|\theta} = 0.$$

It is easy to show that the above two equations imply that the risk premium is zero.

The intuition here is also clear. Infinitely many private signals about asset payoffs imply that informed investors learn the systematic factor perfectly and set their demands such that prices fully reveal the systematic factor F and, thus, eliminate the risk associated with that factor.

General Case

We have considered the special cases where $(b'(\Sigma + \Sigma_s)^{-1}b, b'(\Sigma + \Sigma_\theta)^{-1}b) \rightarrow 0$ and $(b'(\Sigma + \Sigma_s)^{-1}b, b'(\Sigma + \Sigma_\theta)^{-1}b) \rightarrow \infty$. The more interesting case is where the limit of $(b'(\Sigma + \Sigma_s)^{-1}b, b'(\Sigma + \Sigma_\theta)^{-1}b)$ is a non-zero finite constant; what we call finite aggregate precision. This happens, for instance, if $\sqrt{N}b$ converges to a non-zero constant vector when $N \rightarrow \infty$. In effect, under this structure, as the economy expands the informativeness of the private signal for a given asset about the factor decreases. Thus, even though signal noise and idiosyncratic risk are becoming diversified away as the number of assets increases, informed investors' aggregate information about the factor in the limit is imperfect.

The model in our paper is a metaphor to capture the idea that there is residual aggregate uncertainty in the economy. For example, each firm's earnings are influenced by the general health of the economy. However, even if we learn a little bit from each company about the aggregate economy, we would not be able remove the aggregate uncertainty completely. For this reason, the market usually reacts strongly when interest rates are raised or lowered or when government releases statistics about aggregate measurements such as unemployment, GDP, etc. The intuition is that as one expands the number of assets at the same time as one gets more signals about the aggregate economy, the strength of each signal becomes weaker because a firm in a larger economy is relatively less important than a firm in a smaller economy. Our assumption exactly captures this idea—as the economy becomes bigger, what one can learn independently from a single firm about the market is also smaller.[9]

The risk premium in this case is given by the following proposition.

Proposition 2: Given that informed investors receive private signals informative about both idiosyncratic and systematic components of asset payoffs with finite aggregate precision, in the limit as $N \rightarrow \infty$, the risk premium is:

$$E(\bar{\upsilon} - R_f p) = \beta A(u\Sigma_{F|s}^{-1} + (1 - u)\Sigma_{F|\theta}^{-1})^{-1} \frac{\beta'\bar{x}}{N},$$

and the factor risk premium is:

$$\lambda = A(u\Sigma_{F|s}^{-1} + (1 - u)\Sigma_{F|\theta}^{-1})^{-1} \frac{\beta'\bar{x}}{N}.$$

Compared with the no private information case (Equation (21)), it is immediately evident that equilibrium risk premiums only differ with respect to the posterior aggregate uncertainty in the economy; the prior factor precision is transformed into the posterior factor precision $(u\Sigma_{F|s}^{-1} + (1 - u)\Sigma_{F|\theta}^{-1})^{-1}$. Idiosyncratic risks are not priced in the sense that two assets with same beta but different idiosyncratic risks will have the same risk premium. Although information about idiosyncratic shocks affects factor risk premiums, such information does not generate cross-sectional effects on risk premiums because betas are not affected.

To isolate the effect of information asymmetry while controlling for the total information revealed by prices, we can rewrite the posterior factor precision as follows:

$$(\Sigma_{F|s}^{-1} - (1 - u)(\Sigma_{F|s}^{-1} - \Sigma_{F|\theta}^{-1}))^{-1}$$

Interpreting the above expression, when all investors are informed, $\mu = 1$, maximum resolution of uncertainty is achieved and the posterior factor precision is the first term in the outer parentheses, $\Sigma_{F|s}^{-1}$. However, when some investors are not informed, $\mu < 1$, the resolution of uncertainty is diminished by the degree of information asymmetry, measured as $\Sigma_{F|s}^{-1} - \Sigma_{F|\theta}^{-1}$. It follows immediately that, other things being equal, factor risk premiums increase as information asymmetry $\Sigma_{F|s}^{-1} - \Sigma_{F|\theta}^{-1}$ increases. Factor risk premiums also increase in risk aversion and supply of risky assets, a feature preserved from the symmetric information case. We note that even though idiosyncratic risks do not matter in the cross-section for cost of capital, they affect (systematic) factor risk premiums because they act as a source of noise when investors draw inferences about the factors from signals and prices.

Risk Premiums and the Size of the Economy

The speed at which the finite economy approaches the limiting case depends on the information structure of the economy. As previously discussed, in the no information case, the speed of convergence is at the order of $1/N$. Introducing private signals changes the convergence property because a part of the problem is now how the aggregate signal precision converges as N becomes large. Equations (9) and (14) suggest that the systematic information intensity, b, plays an important role in the convergence of the investors' posterior precision to its large N limit. We find that in the interesting case where information aggregation results in finite posterior precision ($\sqrt{N}b$ converges to a constant vector as $N \rightarrow \infty$), the convergence rate for the risk premium will be reduced to $1/\sqrt{N}$.[10]

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[9] As mentioned earlier, an alternative information structure that would preclude learning the factor realization perfectly and preserve our qualitative results is to assume informed investors receive two uncorrelated signals; one about idiosyncratic shocks and the other about the systematic factor. This is similar to the information structure assumed by Admati (1982) in the context of her factor model, the difference being that in our model all informed investors receive the same signals while in Admati (1982), they receive diverse signals.

To visually gauge the speed of convergence, we explicitly calculate the risk premiums for a case where we assume identical distribution for risky asset payoffs with one factor and related signals: i.e., $\Sigma = \sigma^2 I_N$ (I_N is the N-dimensional identity matrix), $\Sigma_s = \sigma_s^2 I_N$, $\Sigma_F = \sigma_f^2$, $\beta = \beta_1 1_{N\times 1}$ ($1_{N\times 1}$ is a $N \times 1$ vector with all elements being 1), $\bar{x} = \bar{x}_1 1_{N\times 1}$ and $b = \frac{k}{\sqrt{N}} 1_{N\times 1}$, where σ, σ_s, σ_f, β_1, $\bar{x}_1$, and k are all constant. Thus, all the covariance matrices are proportional to the identity matrix; the betas of all risky asset payoffs are equal; and the sensitivities of the signals to (for convenience) a single factor are equal. Note that since there is a factor, the distributions of asset payoffs are not independent although they are identical.

Under a set of plausible parameter values where $A = 3$, $\sigma = 30$ percent, $\beta = 1$, $\sigma_f = 20$ percent, $\sigma_s = 25$ percent, $\sigma_{fx} = 30$ percent, $\beta_x = 1$, $\sigma_x = 30$ percent, and $k = -1$, Figure 1 plots the risk premium against the fraction of the informed investors for various numbers of risky assets.[11] The risk premium decreases with N as we would expect. In particular, we observe that there is substantial convergence to the risk premium in the limiting case as the number of assets reaches the hundreds. This suggests that the risk premium in the limit as the number of assets goes to infinity may be a reasonable approximation to the risk premium in a finite economy where the number of assets measures in the thousands.

Deflation by Price

We note that the above results are expressed in terms of dollar risk premiums derived under the standard assumptions of negative exponential utility and normal distributions. To derive results in terms of returns, one can simply deflate both sides of dollar risk premiums by the stock price (e.g., Ou-Yang 2004; Leuz and Verrecchia 2005). An effect of this approach is that the percentage expected return conditional on the current price will be dependent on idiosyncratic information apart from its effects on factor risk premiums because the price depends on idiosyncratic information. Equivalently, conditional risk premiums in percentage returns can be expressed in terms of return betas (i.e., payoff betas scaled by price) and factor risk premiums as depicted in our analysis. Therefore, unlike the previous results based on dollar risk premiums, asymmetric information generates a cross-sectional effect on return risk premiums because return betas are altered by idiosyncratic information.

We caution that characterizing unconditional risk premiums in terms of percentage returns is problematic given division by a random variable that could take on zero as a value. Moreover, it can be argued that in the space of negative exponential utility functions

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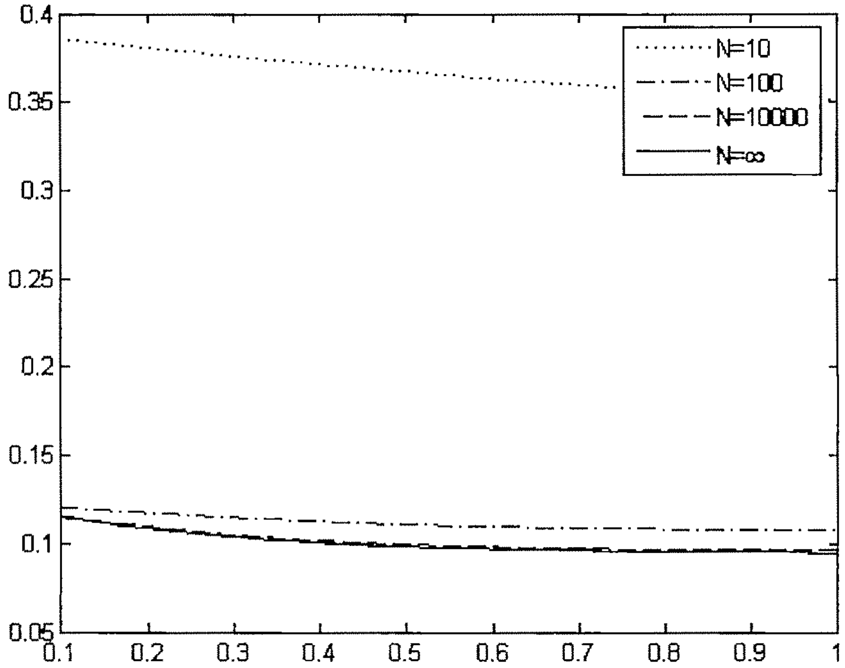
[10] Explicit results are available upon request.

[11] The parameter values for A and β are standard. Typical stock volatility is 30 percent to 50 percent, thus $\sigma = 30$ percent. The values of σ_s, σ_{fx}, and σ_x are chosen in the range of σ. The value of β_x is chosen to be the value of β. The values σ_s, σ_{fx}, and σ_x need to be comparable to the parameter values that describe the underlying returns, otherwise the information effect will be either negligible or overwhelming.

FIGURE 1
The Risk Premium



This graph plots the average risk premium when asset payoffs are identically distributed with one systematic factor. The x-axis is the fraction of informed investors. N is the number of assets. The parameters are: $A = 3$, $\sigma = 30\%$, $\beta = 1$, $\sigma_f = 20\%$, $\sigma_s = 25\%$, $\sigma_{fx} = 30\%$, $\beta_x = 1$, $\sigma_x = 30\%$, and $k = -1$.

risk premiums are more meaningful when expressed in the same dollar terms as asset payoffs. The assumptions of negative exponential utility and normal distributions are adopted in rational expectations models not because of their realism but because of their tractability. Since the absolute risk aversion is a constant for negative exponential utility, the demand for risky securities is independent of investors' wealth. As a result, the risk premium enters the dollar return function in an additive fashion. Division by beginning of the period price introduces idiosyncratic factors since expectations of future cash flows depends on systematic as well as idiosyncratic factors. This is true even in the absence of information asymmetry. Consistent with this argument, most rational expectations models in the literature do not draw implications from deflated prices; risk premiums in this literature are usually understood to be in dollar terms.

Even with the above mentioned caveats, suppose one nevertheless still wants to derive a return specification by deflating the dollar risk premiums using the current stock prices, all cross-sectional effect of information is contained in return betas. After controlling for the return betas, firm-specific information characteristics are again irrelevant for the determination of the cross-section of expected returns.

IV. EMPIRICAL IMPLICATIONS

A common empirical specification of the firm's risk premium is as follows:

$$rp_i = \beta_i\lambda + \gamma x_i,$$

where rp_i is firm i's risk premium, β_i is a vector of the firm's exposures to systematic risk factors, λ is a vector of factor risk premiums, γ is a constant vector of coefficients, and x_i is a vector of firm-specific characteristics such as size, price-to-book ratio, analyst following, etc. While neoclassical asset pricing theory dictates that $\gamma = 0$, implying that the risk premium is completely determined by the product of betas and the factor risk premiums, the question is what changes when information asymmetry is introduced. Our results suggest that β_i does not change in price space, but may change in return space due to deflation effects; λ increases with the increase of information asymmetry (controlling for total information), and γ continues to equal zero.

These results have a number of important empirical implications. First, a common conclusion drawn from Easley and O'Hara's (2004) is that γ ceases to equal zero when information asymmetry is introduced as a consequence of uninformed investors seeking compensation for the risk that they may be trading against informed investors. This explanation is flawed. In Easley and O'Hara (2004), as well as in our model, both informed and uninformed investors exploit liquidity traders whose demands are manifested in an assumption of noisy supply. Compared with informed investors, uninformed investors also have an information advantage over liquidity traders, though this advantage is weakened by noise in prices. The reason γ may be nonzero in Easley and O'Hara (2004) is that investors are exogenously endowed with a limited set of securities such that full diversification cannot take place.

In the real world, of course, the number of securities is large. Nevertheless, one may empirically detect a nonzero γ for a number of plausible reasons. It could be that the investors are not fully diversified as a consequence of behavioral considerations or institutional restrictions. Alternatively, following similar arguments as in Fama and French (1993), it could be that some systematic risk factors are omitted and the information characteristics of interest are correlated with the firm's exposure to the omitted factors (elements of β_i). Finally, it could simply be spurious correlation that has nothing to do with "information risk."[12] Given these prospects, it is difficult to reach an unambiguous conclusion regarding what may be driving empirical findings said to be an artifact of asymmetric information.

Second, no matter whether we consider price space or return space, risk premiums on systematic factors (elements of λ) are affected by information asymmetry. This implication applies in both cross-section and time-series. For example, Bhattacharya et al. (2003), Bhattacharya and Daouk (2002), and Jain (2005) find cross-sectional variation in the average cost of capital across countries where the markets are segmented. They attribute such variation to differences in regulation and capital control. Although these studies cite Easley and O'Hara (2004) for theoretical motivation, we believe our model provides a more direct theoretical explanation for such empirical findings. Our analysis suggests that similar market-level analysis can be done in time-series, though power is likely to be an issue since there may be few significant disclosure regime changes in any one particular country.

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[12] Recent papers by Cohen (2004), Nikolaev and van Lent (2005), Chen et al. (2004) address this concern and find the correlation between cost of capital and information asymmetry becomes insignificant when endogeneity and correlated omitted variables are considered.

Third, our theory suggests that in price space there should be no cross-sectional effect of asymmetric information, while in return space there could be a cross-sectional effect due entirely to deflation by price. The existing empirical papers do not investigate the information effect in price space. Hence, our results suggest that this is a promising arena for further inquiry.

Finally, since all systematic factors are exogenously given, our model is silent on the choice of systematic risk factors. Therefore our theory is not inconsistent with studies that presume an existence of a systematic"information risk" factor (e.g., Aboody et al. 2005; Francis et al. 2004b; Easley et al. 2002). Our study, however, highlights the *ad hoc* nature of this presumption.

V. CONCLUSION

Our objective in this study is to contribute to the development of a theoretical foundation for empirical inquiries about the relation between asymmetric information and cost of capital. We assume a factor structure for both asset payoffs and private signals in the context of a competitive noisy rational expectations model of market behavior. This structure allows us to consider the effects of private information about systematic factors as well as about idiosyncratic shocks on risk premiums and, hence, on cost of capital.

Taking the large economy limit, we show that equilibrium risk premiums under asymmetric information are entirely determined by the product of betas and factor risk premiums as in an economy without private signals. This is true whether risk premiums are measured in dollars, as is the convention when assuming normal distributions and exponential utility, or percentage returns after scaling by price. The risk associated with idiosyncratic shocks affects risk premiums only as a source of noise in drawing inferences about systematic factors from private signals; idiosyncratic risk is eliminated through diversification. Greater information asymmetry about systematic factors (holding total information constant) leads to less resolution of uncertainty, all else held constant, and, hence, higher factor risk premiums.

Although information asymmetry does not affect cost of capital in the cross-section, it affects market-wide factor risk premiums. Accordingly, more promising avenues for investigating the effects of asymmetric information would appear to be at the aggregate market level, rather than the firm level as in most existing empirical studies.

Similar to Ross's (1976) Arbitrage Pricing Theory, a limitation of our model is that all systematic risk factors are exogenously given. As such, the model has limited power in prescribing factors to be used empirically. In particular, our analysis is silent on the existence of a systematic factor related to asymmetric information. Investigating conditions under which such a factor might emerge in equilibrium is a topic we commend to future research.

APPENDIX

In the Appendix, we will use the following identity extensively:

$$(\Sigma + \beta\Omega\beta')^{-1} = \Sigma^{-1} - \Sigma^{-1}\beta(\Omega^{-1} + \beta'\Sigma^{-1}\beta)^{-1}\beta'\Sigma^{-1}$$

The Proof of Remark 1

We solve for the filtering rule, given signal s. Our assumptions have specified the distribution functions $f(\upsilon|F, s)$, $f(\upsilon|F)$ and $f(F)$. Therefore:

$$f(\upsilon,F,s) = f(s|\upsilon,F)f(\upsilon|F)f(F).$$

We can rewrite the above as:

$$f(\upsilon,F,s) = f(\upsilon|s,F)f(F|s)f(s).$$

Focusing on the exponential terms of the joint normal distribution densities, we obtain:

$$-\ln f(\upsilon,F,s) = -\ln f(s|\upsilon,F) - \ln f(\upsilon|F) - \ln f(F)$$

$$\frac{1}{2}(s - (\upsilon - \bar{\upsilon} - \beta F) - bF)'\Sigma_s^{-1}(s - (\upsilon - \bar{\upsilon} - \beta F) - bF)$$

$$+ \frac{1}{2}(s - (\upsilon - \bar{\upsilon} - \beta F))'\Sigma^{-1}(s - (\upsilon - \bar{\upsilon} - \beta F)) + \frac{1}{2}F'\Sigma_F^{-1}F$$

$$= \frac{1}{2}(\upsilon - \bar{\upsilon} - \beta F)'(\Sigma^{-1} + \Sigma_s^{-1})(\upsilon - \bar{\upsilon} - \beta F) - (\upsilon - \bar{\upsilon} - \beta F)\Sigma_s^{-1}(s - bF)$$

$$+ \frac{1}{2}(s - bF)'\Sigma_s^{-1}(s - bF) + \frac{1}{2}F'\Sigma_F^{-1}F$$

$$= \frac{1}{2}(\upsilon - \mathrm{E}[\upsilon|s,F])'\Sigma_{\upsilon|s,F}^{-1}(\upsilon - \mathrm{E}[\upsilon|s,F])$$

$$+ \frac{1}{2}(s - bF)'(\Sigma - \Sigma_s)^{-1}(s - bF) + \frac{1}{2}F'\Sigma_F^{-1}F$$

$$= \frac{1}{2}(\upsilon - \mathrm{E}[\upsilon|s,F])'\Sigma_{\upsilon|s,F}^{-1}(\upsilon - \mathrm{E}[\upsilon|s,F]) + \frac{1}{2}s'(\Sigma + \Sigma_s)^{-1}s$$

$$+ \frac{1}{2}(bF)'(\Sigma + \Sigma_s)^{-1}(bF) - s(\Sigma + \Sigma_s)^{-1}bF + \frac{1}{2}F'\Sigma_F^{-1}F$$

$$= \frac{1}{2}(\upsilon - \mathrm{E}[\upsilon|s,F])'\Sigma_{\upsilon|s,F}^{-1}(\upsilon - \mathrm{E}[\upsilon|s,F])$$

$$+ \frac{1}{2}(F - \mathrm{E}[F|s])'\Sigma_{F|s}^{-1}(F - \mathrm{E}[F|s]) + \frac{1}{2}s'\hat{\Sigma}_s^{-1}s.$$

The distribution functions $f(\upsilon|F,s)$, $f(F|s)$, and $f(s)$ can then be identified from the above equation, with:

$$\mathrm{E}(\upsilon|s,F) = \bar{\upsilon} + \beta F + \Sigma_{\upsilon|s,F}\Sigma_s^{-1}(s - bF)$$

$$\mathrm{E}(F|s) = \Sigma_{F|s}b'(\Sigma + \Sigma_s)^{-1}s$$

$$\Sigma_{\upsilon|s,F}^{-1} = \Sigma^{-1} + \Sigma_s^{-1}$$

$$\Sigma_{F|s}^{-1} = \Sigma_F^{-1} + b'(\Sigma + \Sigma_s)^{-1}b$$

$$\hat{\Sigma}_s^{-1} = \Sigma + b'\Sigma_F b + \Sigma_s.$$

The Proof of Remark 2

The structure of the filtering rule, given signal θ, is the same as that for s. The proof proceeds in exactly the same fashion.

Proof of Proposition 1

Because $b = 0$, we have:

$$\Sigma_{F|s} = \Sigma_F;$$
$$\Sigma_{\upsilon|s} = \Sigma_{\upsilon|s,F} + \beta\Sigma_F\beta';$$
$$\Sigma_{F|\theta} = \Sigma_F;$$
$$\Sigma_{\upsilon|\theta} = \Sigma_{\upsilon|\theta,F} + \beta\Sigma_F\beta'.$$

Intuitively, the matrices $\Sigma_{\upsilon|s}$ and $\Sigma_{\upsilon|\theta}$ differ only in the idiosyncratic matrices $\Sigma_{\upsilon|s,F}$ and $\Sigma_{\upsilon|\theta,F}$, which do not matter for the risk premium and thus should produce the risk premium $A\beta\Sigma_F\beta'\bar{x}$. The formal proof is as follows. From:

$$\Sigma_\theta = \Sigma_s + \lambda(\beta_x\Sigma_{Fx}\beta_x' + \Sigma_x)\lambda' \geq \Sigma_s,$$

we know that:

$$\Sigma \geq (\Sigma^{-1} + \Sigma_\theta^{-1})^{-1} = \Sigma_{\upsilon|\theta,F}^{-1} \geq (\Sigma^{-1} + \Sigma_s^{-1})^{-1} = \Sigma_{\upsilon|s,F}^{-1}.$$

It follows that:

$$\begin{aligned}\Sigma + \beta\Sigma_F\beta' &= (u(\Sigma + \beta\Sigma_F\beta')^{-1} + (1-u)(\Sigma + \beta\Sigma_F\beta')^{-1})^{-1}\\ &\geq (u(\Sigma_{\upsilon|s,F} + \beta\Sigma_F\beta')^{-1} + (1-u)(\Sigma_{\upsilon|\theta,F} + \beta\Sigma_F\beta')^{-1})^{-1}\\ &= \bar{\Sigma}_\upsilon \geq (u(\Sigma_{\upsilon|s,F} + \beta\Sigma_F\beta')^{-1} + (1-u)(\Sigma_{\upsilon|s,F} + \beta\Sigma_F\beta')^{-1})^{-1}\\ &= \Sigma_{\upsilon|s,F} + \beta\Sigma_F\beta'.\end{aligned}$$

Hence, we find the upper and lower bounds for $\lim_{N\to\infty} \frac{1}{N} \bar{\Sigma}_\upsilon$:

$$\lim_{N\to\infty} \frac{1}{N}(\Sigma + \beta\Sigma_F\beta') = \lim_{N\to\infty} \frac{1}{N}\beta\Sigma_F\beta' \geq \lim_{N\to\infty} \frac{1}{N}\hat{\Sigma}_\upsilon$$
$$\geq \lim_{N\to\infty} \frac{1}{N}(\Sigma_{\upsilon|s,F} + \beta\Sigma_F\beta') = \lim_{N\to\infty} \frac{1}{N}\beta\Sigma_F\beta'.$$

Therefore, the average risk premium is:

$$E(\bar{\upsilon} - R_f p) = A\bar{\Sigma}_\upsilon\bar{x}/N \to A(\Sigma_{\upsilon|s,F} + \beta\Sigma_F\beta')\bar{x}/N \to \beta A\Sigma_F\beta'\bar{x}/N.$$

Proof of Proposition 2

For the case of non-identically distributed risky asset payoffs, the leading order terms in the large N limit are:

$$\Sigma_{\upsilon|s,F} = (\Sigma^{-1} + \Sigma_s^{-1})^{-1}$$

$$\Sigma_{F|s} = (\Sigma_F^{-1} + b'(\Sigma + \Sigma_s)^{-1}b)^{-1}$$

The variance of v conditional on s:

$$\Sigma_{\upsilon|s} = \Sigma_{\upsilon|s,F} + \beta\Sigma_F\beta' + O(N^{-1/2}),$$

$$\Phi_s = \Sigma_{\upsilon|s,F}\Sigma_s^{-1} + \frac{1}{\sqrt{N}}\beta\Sigma_{F|s}k'(\Sigma + \Sigma_s)^{-1} + O(N^{-1}).$$

Both first terms in the above equations are diagonal matrices. The second terms are due to factors. We use $O(N^a)$ to denote matrices with all of their elements generally non-zero and of order N^a. In the case of identical assets, $O(N^\alpha) \propto N^a 1_{N\times N}$. These terms will be negligible, in the large N limit, as far as the risk premium is concerned. The Φ_s^{-1} matrix is:

$$\begin{aligned}\Phi_s^{-1} &= \Sigma_s\left(I_N + \frac{1}{\sqrt{N}}\Sigma_{\upsilon|F,s}^{-1}\beta\Sigma_{F|s}k'(\Sigma_s^{-1}\Sigma + I_N)^{-1}\right)^{-1}\Sigma_{\upsilon|s,F}^{-1}\\ &= \Sigma_s(I_N - \Sigma_{\upsilon|F,s}^{-1}\beta k'(\sqrt{N}\Sigma_{F|s}^{-1} + k'(\Sigma_s^{-1}\Sigma + I_N)\Sigma_{\upsilon|F,s}^{-1}\beta)^{-1}k'(\Sigma_s^{-1}\Sigma + I_N)^{-1})\Sigma_{\upsilon|s,F}^{-1}\end{aligned}$$

and:

$$\begin{aligned}\Phi_s^{-1}\Sigma_{\upsilon|s} &= \Sigma_s\left(I_N + \frac{1}{\sqrt{N}}\Sigma_{\upsilon|F,s}^{-1}\beta\Sigma_{F|s}k'(\Sigma_s^{-1}\Sigma + I_N)^{-1}\right)^{-1}\Sigma_{\upsilon|s,F}^{-1} \times (I_N + \Sigma_{\upsilon|s,F}^{-1}\beta\Sigma_{F|s}\beta')\\ &= \Sigma_s\begin{pmatrix}I_N - \Sigma_{\upsilon|F,s}^{-1}\beta k'(\sqrt{N}\Sigma_{F|s}^{-1} + k'(\Sigma_s^{-1}\Sigma + I_N)\Sigma_{\upsilon|F,s}^{-1}\beta)^{-1}k'(\Sigma_s^{-1}\Sigma + I_N)^{-1}\\ - \Sigma_{\upsilon|F,s}^{-1}\beta k'(\sqrt{N}\Sigma_{F|s}^{-1} + k'(\Sigma_s^{-1}\Sigma + I_N)\Sigma_{\upsilon|F,s}^{-1}\beta)^{-1}\sqrt{N}\beta\end{pmatrix}\\ &\to \Sigma_s\left(I_N + \frac{1}{\sqrt{N}}\Sigma_{\upsilon|F,s}^{-1}\beta\left(\frac{1}{N}k'\Sigma\beta\right)^{-1}\beta'\right)^{-1}.\end{aligned}$$

Therefore:

$$\lambda = \frac{A}{uN}\Phi_s^{-1}\Sigma_{\upsilon|s} = \frac{1}{\sqrt{N^3}}Au^{-1}\Sigma_s\Sigma_{\upsilon|s,F}^{-1}\beta\left(\frac{1}{N}k'\Sigma^{-1}\beta\right)^{-1}\beta'.$$

The signal θ is now:

$$\theta = s - \frac{1}{\sqrt{N}}\frac{A}{N}u^{-1}\Sigma_s\Sigma_{\upsilon|s,F}^{-1}\beta\left(\frac{1}{N}k'\Sigma^{-1}\beta\right)^{-1}\frac{\beta'\beta_x}{N}F_x = s - \frac{1}{\sqrt{N}}\Lambda F_x$$

with $\Lambda = \frac{A}{N}u^{-1}\Sigma_s\Sigma_{\upsilon|s,F}^{-1}\beta\left(\frac{1}{N}k'\Sigma^{-1}\beta\right)^{-1}\frac{\beta'\beta}{N}$. The idiosyncratic component of the random supply disappears; it is diversified away. The covariance matrix of the payoffs, conditional on θ, is:

$$\Sigma_\theta = \Sigma_s + \frac{1}{N}\Lambda\beta_x\Sigma_{Fx}\beta_x'\Lambda'.$$

Note that Σ_s is a diagonal matrix while $\Lambda\beta_x\Sigma_{Fx}\beta_x'\Lambda'$ is a matrix with all of its matrix elements being of order 1. Therefore, when Σ_θ is multiplied by a vector of 1's from the right, the second term has the same order of magnitude as the first term. We can show that:

$$\Sigma_{\upsilon|\theta,F} = \Sigma_{\upsilon|s,F} + O(N^{-1}).$$

As will be shown later, the contribution of such terms to the risk premium goes to zero in the limit as $N \to \infty$. The factor covariance matrix, conditional on θ, is:

$$\Sigma_{F|\theta}^{-1} = \Sigma_F^{-1} + \frac{1}{N}k'\left(\Sigma + \Sigma_s + \frac{1}{N}\Lambda\beta_x\Sigma_{Fx}\beta_x'\Lambda'\right)^{-1} k.$$

Note that, when multiplied by vectors of 1's from left and from right, the term $\frac{1}{N}\Lambda\beta_x\Sigma_{Fx}\beta_x'\Lambda'$ produces a $K \times K$ matrix with elements of order N, the same as $\Sigma + \Sigma_s$.

The variance of ν, conditional on θ, is:

$$\Sigma_{\upsilon|\theta} = \Sigma_{\upsilon|s,F} + \beta\Sigma_{F|\theta}\beta'$$

The matrix $\Sigma_{\upsilon|s,F}$ is diagonal, while all the elements of the matrix $\beta\Sigma_{F|\theta}\beta'$ are of order 1. The terms neglected earlier produce matrices with all elements of order N^{-1}.

From the identity:

$$\begin{aligned} &u\Sigma_{\upsilon|s}^{-1} + (1-u)\Sigma_{\upsilon|\theta}^{-1} \\ &\quad = \Sigma_{\upsilon|s,F}^{-1} - \Sigma_{\upsilon|s,F}^{-1}\beta(u(\Sigma_{F|s}^{-1} + \beta'\Sigma_{\upsilon|F,s}^{-1}\beta)^{-1} + (1-u)(\Sigma_{F|\theta}^{-1} + \beta'\Sigma_{\upsilon|F,\theta}^{-1}\beta)^{-1})\beta'\Sigma_{\upsilon|s,F}^{-1}, \end{aligned}$$

we can write:

$$(u\Sigma_{\upsilon|s}^{-1} + (1-u)\Sigma_{\upsilon|\theta}^{-1})^{-1} = \Sigma_{\upsilon|s,F} + \beta M^{-1}\beta',$$

where:

$$\begin{aligned} M &= (u(\Sigma_{F|s}^{-1} + \beta'\Sigma_{\upsilon|F,s}^{-1}\beta)^{-1} + (1-u)(\Sigma_{F|\theta}^{-1} + \beta'\Sigma_{\upsilon|F,\theta}^{-1}\beta)^{-1})^{-1} - \beta'\Sigma_{\upsilon|s,F}\beta \\ &= (u(\Sigma_{F|s}^{-1} + \beta'\Sigma_{\upsilon|F,s}^{-1}\beta)^{-1} + (1-u)(\Sigma_{F|\theta}^{-1} + \beta'\Sigma_{\upsilon|F,\theta}^{-1}\beta)^{-1})^{-1} \\ &\quad \times (u(\Sigma_{F|s}^{-1} + \beta'\Sigma_{\upsilon|F,s}^{-1}\beta)^{-1}\Sigma_{F|s}^{-1} + (1-u)(\Sigma_{F|\theta}^{-1} + \beta'\Sigma_{\upsilon|F,\theta}^{-1}\beta)^{-1}\Sigma_{F|\theta}^{-1}). \end{aligned}$$

In the large N limit, $\beta'\Sigma_{\upsilon|F,s}^{-1}\beta$ is of order N, therefore, $\Sigma_{F|s}^{-1} + \beta'\Sigma_{\upsilon|F,s}^{-1}\beta \to \beta'\Sigma_{\upsilon|F,s}^{-1}\beta$. Similarly, $\Sigma_{F|\theta}^{-1} + \beta'\Sigma_{\upsilon|F,\theta}^{-1}\beta \to \beta'\Sigma_{\upsilon|F,\theta}^{-1}\beta$, so:

$$\begin{aligned} M &\to \beta'\Sigma_{\upsilon|F,\theta}^{-1}\beta(u(\beta'\Sigma_{\upsilon|F,s}^{-1}\beta)^{-1}\Sigma_{F|s}^{-1} + (1-u)(\Sigma_{F|\theta}^{-1} + \beta'\Sigma_{\upsilon|F,\theta}^{-1}\beta)^{-1}\Sigma_{F|\theta}^{-1}) \\ &= u\Sigma_{F|s}^{-1} + (1-u)\Sigma_{F|\theta}^{-1}. \end{aligned}$$

The risk premium is given by:

$$\mathrm{E}(\bar{\upsilon} - R_f p) = \beta A(u\Sigma_{F|s}^{-1} + (1-u)\Sigma_{F|\theta}^{-1})^{-1}\frac{\beta'\bar{x}}{N},$$

and the factor risk premium is:

$$\lambda = A(u\Sigma_{F|s}^{-1} + (1 - u)\Sigma_{F|\theta}^{-1})^{-1} \frac{\beta'\bar{x}}{N}.$$

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THE ACCOUNTING REVIEW
Vol. 82, No. 3
2007
pp. 731–758

Information Pursuit in Financial Statement Analysis: Effects of Choice, Effort, and Reconciliation

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ABSTRACT: Prior research provides evidence that information affects financial statement users' judgments less when that information is provided in a less accessible format (e.g., information disclosed in a footnote or less prominent financial statement rather than being recognized on the income statement [Maines and McDaniel 2000]). We provide evidence that, conditional on users performing the analysis necessary to transform the financial statements to appear as if disclosed information had been recognized, that information may affect users' judgments *more* than it would have if it had been recognized initially. Our experiments are set in the context of constructive capitalization of operating leases. The first experiment manipulates three variables that we hypothesize will contribute to this effect: choice to use transformed financial statements, effort spent on the transformation process, and reconciliation of pre- and post-transformation numbers. We provide evidence that, in the constructive-capitalization setting we operationalize, information has a greater effect on judgments when effort was expended to obtain the information and the information is displayed in a reconciled format. The second experiment focuses on the effort effect and replicates it with additional controls. These results have implications for standard-setters who consider the relative benefits of recognition and disclosure, and for financial statement users who transform financial statements as part of their analyses.

Keywords: *lease; recognition; disclosure; choice; effort; disaggregation; reconciliation; fixation; salience; financial statement analysis.*

We thank Rob Bloomfield, Brooke Elliott, Pat Hopkins, Marlys Lipe, Laureen Maines, Jamie Pratt, Shana Clor-Proell, Kathy Rupar, J. Reed Smith, Geoff Sprinkle, Hun-Tong Tan, Jim Wahlen, Jeff Wilks, two anonymous reviewers, and workshop participants at Cornell University, Harvard University, Indiana University, The Pennsylvania State University, The University of Tennessee, the 2005 Brigham Young University Accounting Research Symposium for comments, and Cornell's Johnson Graduate School of Management for financial support.

Editor's note: This paper was accepted by Marlys Lipe.

Submitted January 2006
Accepted September 2006

I. INTRODUCTION

Much prior research suggests that the format in which information is communicated affects the extent to which the information affects users' judgments. In general, information affects user judgments more when the information is recognized in an income statement or balance sheet than when it is disclosed in a footnote or in a less prominent financial statement. These "format effects" may occur because disclosure signals to users that information is less useful (Cotter and Zimmer 2003; Liang and Zhang 2006), consistent with standard-setters' rationale for relegating information to disclosure (SFAC No. 5, Financial Accounting Standards Board [FASB] 1984). However, even if disclosed information is useful, users may not consider it because human information-processing limitations render disclosed information less accessible (Maines and McDaniel 2000). One approach to helping users consider the implications of disclosed information is to transform the financial statements to appear as if the information had been recognized (Revsine et al. 2004). An express purpose of some disclosures is to allow that sort of transformation (see SFAS No. 123 [FASB 1995] with respect to stock options).

Our paper examines how user judgments are influenced by such transformational financial statement analysis techniques. Whereas prior research finds that disclosed information has less of an effect on user judgments than does recognized information, we predict that, conditional on disclosures being transformed to appear as if they have been recognized, disclosures can have a *greater* effect on users' judgments than does information that is recognized to begin with, because transformation requires an explicit choice to pursue information, entails effortful processing, and provides a reconciliation between pre- and post-transformation numbers.

Testing our hypotheses requires that we examine judgments of users who possess knowledge of a transformational financial statement analysis skill. Therefore, we performed two experiments using as participants M.B.A. students who previously had been taught constructive capitalization of operating leases using a procedure adapted from Imhoff et al. (1991). In both experiments, the participants' task was to assess the relative riskiness of two companies in the franchise-restaurant industry, with one company (Luby's, Inc.) having no operating leases, and the other company (P. F. Chang's China Bistro, Inc. [hereafter, Chang's]) having significant amounts of operating leases. An important element of financial statement analysis for this task is to enhance comparability between the two firms by transforming Chang's balance sheet by constructively capitalizing operating leases.

Experiment One has five conditions in a 2 × 2 + 1 between-subjects design. The 2 × 2 portion of the design manipulates "choice" (i.e., whether participants are automatically given the effects of constructive capitalization or must choose to get that information) and "effort" (i.e., whether participants have to use a template to perform the calculations necessary to constructively capitalize operating leases or have the results of those calculations provided to them). All four of these cells are "*high reconciliation*," in that participants are provided a reconciliation that includes pre-adjustment, adjustment, and post-adjustment numbers. The +1 portion of the design is a benchmark "low choice, low effort, *low reconciliation*" condition in which financial information is presented as if Chang's operating leases have already been capitalized (i.e., participants see only the post-transformation amounts).

In this design, the benchmark "low choice, low effort, low reconciliation" cell depicts a pure recognition setting in which all leases are capitalized, the "high choice, high effort, high reconciliation" cell depicts the setting faced currently by a financial statement user who chooses to exert the effort necessary to constructively capitalize operating leases, and the other cells depict intervening conditions that are possible between those two extremes.

Comparisons between the low- and high-choice conditions (holding constant high reconciliation) reveal effects of choice, comparisons between the low- and high-effort conditions (holding constant high reconciliation) reveal effects of effort, and comparisons between the low- and high-reconciliation conditions (holding constant low choice and low effort) reveal effects of reconciliation.

Results of Experiment One indicate that participants' risk judgments were significantly affected by the effort and reconciliation manipulations, but not by the choice manipulation. Specifically, participants assessed Chang's as relatively more risky when participants performed the constructive capitalization themselves (and thus exerted effort, rather than simply being provided the effects of constructive capitalization) and when pre- and post-transformation financial statements were reconciled (thus highlighting the effects of constructive capitalization). Debriefing data suggest that these effects occurred at least in part because effort and reconciliation affected participants' perceptions of the extent to which they were well informed.

Experiment Two is designed to further test the effort effects identified in Experiment One. In Experiment One, low-effort participants were provided the results of constructive capitalization, but not the entire completed constructive-capitalization template, so that those participants could complete the template during debriefing to provide data for a knowledge check. However, this allows the possibility that differences in risk ratings attributed to effort in Experiment One could instead be caused by differences in access to information contained in the constructive-capitalization template. Therefore, Experiment Two holds constant participants' access to the constructive-capitalization template, and varies in a 1 × 2 between-subjects design whether participants perform the calculations in the template or are provided the completed template. Results replicate the effort effect. Additional data indicate that participants in the high-effort condition spent more time on the risk-analysis task, again suggesting that the higher risk ratings observed in that condition are due to participants expending greater effort.

These results contribute to several literatures. First, regarding format effects, our results indicate that it is not always the case that recognized information affects judgment more than does disclosed information. In circumstances where transformational financial statement analysis requires significant effort or reconciles disaggregated information, disclosed information may affect judgments more than it would have had it been recognized initially. Ironically, while the information may have been disclosed rather than recognized because it is less useful, the effortful processing necessary for that information to be considered, and the consequential disaggregated reconciliation generated by transformational analysis, might result in the information affecting judgment to a greater extent than it would have had it been recognized initially.

Our results also have implications for Maines and McDaniel's (2000) framework for considering the effects of format on user judgments. Maines and McDaniel (2000) highlight that disclosed information is less likely to affect judgment when effortful processing is necessary to access the information. Our results complement theirs by indicating that, conditional on cognitive effort being expended, disclosed information can be more likely to affect judgment.

Given that transformational financial statement analysis techniques could be implemented via decision aids, our results also have implications for predicting when decision-aid implementations will be successful. Prior research suggests that decision-aid reliance depends on the balance between a user's confidence in their unaided judgment and their confidence in the aid (Eining et al. 1997; Nelson et al. 2003). Our results indicate that effort and reconciliation also play a role in reliance. Providing information (e.g., via a decision aid) rather than requiring production of information, or making disclosed information more accessible via search-facilitating technology (Hodge et al. 2004), could affect the extent to which that information is relied upon in subsequent decision making.

Our results have implications for the ongoing consideration by accounting standard-setters about whether lessees should recognize or disclose assets and liabilities associated with operating leases. As discussed further in Section II, the Securities and Exchange Commission (SEC 2005) has expressed concern that many arrangements are structured to obtain the off-balance-sheet treatment allowed for operating leases. With the SEC's encouragement, the FASB has added a comprehensive leasing project to its agenda. While prior research suggests that one effect of requiring recognition of operating leases would be that users are more likely to attend to information about operating leases, our study suggests that another effect would be that the judgments of those users who constructively capitalize operating leases are not influenced by the amount of effort expended on constructive capitalization.

The effects of effort and reconciliation that we identify for the leasing context could also occur in other financial statement analysis contexts in which users perform transformational analyses. For example, users might choose to transform financial statements to account for differences in stock compensation accounting, inventory-cost-flow assumptions, revenue-recognition principles, pension and healthcare assumptions and funded status, asset securitization accounting, transitory income recognition, etc. Future research could examine how our results generalize to other contexts that require more or less effortful transformations.

Our results also contribute to the psychology literature. Prior psychology studies provide evidence that information affects judgment more when people choose to pursue the information (Bastardi and Shafir 1998; Redelmeier et al. 2001). Our results indicate that, regardless of choice, information affects judgment more when people expended effort to obtain it, at least in some settings. We do not find that explicit choice to pursue information affects subsequent judgment, but that lack of effect could be due to participants in our experiment viewing their choice as being noncontroversial. Another useful direction for future accounting research is to further consider how choice to pursue information affects subsequent use of that information.

The rest of this paper proceeds as follows. Section II provides background and our hypotheses. Section III describes the experimental method used in Experiment One, and Section IV states the results of that experiment. Section V describes the method and results of Experiment Two. Section VI provides a summary of results and discusses implications, limitations, and directions for future research.

II. BACKGROUND

Format Effects

Prior archival research suggests that financial statement users' decisions are affected to some extent by information that is only disclosed in footnotes,[1] but that users' decisions are affected more by information when it is recognized than when it is disclosed.[2] Prior experimental research likewise provides evidence that information affects user judgments more when it is recognized or disclosed more prominently.[3]

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[1] For reviews, see Barth et al. (2001) and Holthausen and Watts (2001); for evidence concerning leases, see Bowman (1980), Imhoff et al. (1993, 1995), Ely (1995), and Beattie et al. (2000).

[2] See Davis-Friday et al. (1999), Davis-Friday et al. (2004), and Picconi (2005) with respect to post-retirement benefits, Espahbodi et al. (2002) with respect to stock compensation expense, and Cotter and Zimmer (2003) with respect to asset revaluations. See Bernard and Schipper (1994) for discussion of design challenges with respect to this question.

One reason that disclosed information could have less of an effect on user judgments than recognized information is that users believe that disclosed information is less useful, consistent with the FASB's criteria for determining when information should be recognized or disclosed (SFAC No. 5). Liang and Zhang (2006) theorize that accounting systems that allow for flexibility in the recognition/disclosure decision may induce more efficient financing decisions because they allow for accounting choice to convey additional information regarding information usefulness. Supporting this viewpoint, Cotter and Zimmer (2003) provide evidence that investors react less to Australian asset revaluations that are disclosed than to those that are recognized, and that firms tend to choose to disclose asset revaluations when asset-value estimates are less reliable, such that investors appear to be correctly viewing recognition versus disclosure as a signal about information reliability. Also, Libby et al. (2006) provide evidence that auditors tolerate more misstatement in disclosed numbers than in recognized numbers, which is consistent with investors correctly viewing disclosed numbers as less reliable.

However, format differences like recognition versus disclosure could also affect judgment because of investors' information-processing limitations. Maines and McDaniel (2000) provide a framework that is useful for considering possible explanations for how format affects judgment. They suggest that information must be acquired (i.e., perceived), evaluated (i.e., assessed in terms of its implication for the decision in question), and weighted (i.e., relied upon to some extent) for that information to affect a user's decision. They further suggest that differences in format potentially affect each of these decision stages in two main ways: signaling whether information is useful (e.g., by placement in a performance statement, using key jargon like "income," and explicitly tying the information to key financial statement elements), and affecting cognitive costs (e.g., isolating relevant information on its own line and not requiring additional calculations to disaggregate it from other information). Maines and McDaniel (2000) predict and find that their M.B.A. student-subjects acquire and evaluate comprehensive income information regardless of format, but that they weight it more heavily if it is presented in a format that signals importance and reduces cognitive costs.

Other research provides evidence that format affects judgment via information acquisition as well as weighting. For example, Hirst et al. (2004) predict and find that even sophisticated users are less likely to acquire and use fair value data that are footnote disclosed than data recognized in the income statement. After controlling for information acquisition, Hirst et al. (2004, 469) find no effect of format. Hodge et al. (2004) predict that XBRL search technology helps M.B.A. students to acquire and integrate (i.e., evaluate and weight) stock compensation information presented in a disclosure format. Only about half of Hodge et al.'s (2004) subjects used the XBRL technology when they had access to it, but those who did were more likely to acquire and use disclosed information. After controlling for acquisition, Hodge et al. (2004, footnote 14) provide somewhat weaker evidence that XBRL affects integration (footnote 14).

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[3] See Munter and Ratcliffe (1983), Wilkins and Zimmer (1983a, 1983b), Wilkins (1984), and Gopalakrishnan and Parkash (1996) with respect to operating leases, Hodge et al. (2004) and Frederickson et al. (2006) with respect to employee stock options, Harper et al. (1987) and Sami and Schwartz (1992) with respect to pensions, Harper and Mister (1991) with respect to post-retirement health care benefits, Hopkins (1996) with respect to hybrid securities, Kennedy et al. (1998) with respect to contingent liabilities, Hirst and Hopkins (1998), Maines and McDaniel (2000), and Bloomfield et al. (2006) with respect to comprehensive income, Hopkins et al. (2000) with respect to business combinations, Luft and Shields (2001) with respect to intangibles, Dietrich et al. (2001) with respect to oil reserves, and Hirst et al. (2003) with respect to fair-value accounting.

Overall, these and similar studies suggest that: (1) format can affect user judgments, regardless of user sophistication, (2) at least some of this effect can be attributed to users failing to acquire information that is presented in less transparent formats due to the high cognitive costs of doing so, and (3) users may also weight the disclosed information they do acquire to a lesser extent than recognized information because they perceive disclosure as signaling less information usefulness. Assuming that users acquire disclosed information and deem it sufficiently useful to merit consideration, they can transform financial statements to treat disclosed information as if it had been recognized. For example, financial statement analysis texts (e.g., Revsine et al. 2004) often encourage application of techniques that treat stock compensation as if it had been expensed or that "constructively capitalize" operating leases to put assets and liabilities on the balance sheet.

Information Pursuit

Our paper focuses on how users' judgments are affected by financial statement analysis techniques that transform financial statements to treat disclosed information as if it had been recognized. We draw on prior research in psychology to predict that information acquired in this manner can have a *greater* effect on judgment than it would have had if it been recognized initially.

Specifically, prior research in psychology suggests that pursuit of information affects the weighting of that information in decision making, regardless of the actual usefulness of the information (Bastardi and Shafir 1998; Redelmeier et al. 2001). For example, in study two of Redelmeier et al. (2001), 461 practicing physicians were given a written scenario describing an ailing patient and were asked to recommend either surgery or radiation therapy as a treatment. Half of the doctors were assigned to a one-stage treatment in which they received a set of information in the scenario paragraph along with an additional sentence stating information obtained through a medical consultation with the patient. The other half of the doctors were assigned to a two-stage treatment in which they first received only the scenario paragraph and chose whether they would like to obtain information from a medical consultation before deciding between surgery and radiation therapy. Those choosing to obtain the additional information were then provided the remainder of the information set (the final sentence received by the first group of doctors). The patient consultation data indicated an increased risk of surgery due to chronic emphysema. Overall, doctors in the two-stage treatment appeared to weight the additional information more heavily than did doctors in the one-stage treatment, and thus were more likely to recommend against surgery (58 percent of all doctors in the one-stage treatment recommended against surgery versus 74 percent of all doctors in the two-stage treatment, with 92 percent of those doctors in the two-stage treatment who chose to have the medical consultation recommending against surgery).[4]

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[4] Judgments made by participants in the studies of Bastardi and Shafir (1998) and Redelmeier et al. (2001) were binary; and these studies view information as "instrumental" if, all else equal, the presence or absence of the information affects the binary outcome. These studies suggest that information that is noninstrumental when provided can become instrumental if pursued. As noted below, the judgment in our study is not binary, so we avoid making claims about instrumentality of information. Instead, we focus on a continuous judgment, and the degree to which pursued versus provided information affects a final judgment.

Bastardi and Shafir (1998) and Redelmeier et al. (2001) note that, similar to preference reversals, this "information-pursuit bias" is irrational, violating the assumption of procedural invariance (Slovic and Lichtenstein 1983).[5] Bastardi and Shafir (1998) demonstrate information-pursuit bias by college students facing a variety of common decision scenarios, while Redelmeier et al. (2001) demonstrate it with highly experienced practicing clinicians (dialysis nurses, urologists, and academic physicians) facing medical scenarios.

Bastardi and Shafir (1998) and Redelmeier et al. (2001) describe their findings as consistent with both Bem's (1978) self-perception theory and Festinger's (1957) cognitive dissonance theory. Bem (1978) provides evidence that people who are uncertain of their attitude infer their attitude from other cues, including their own behavior. Thus, knowledge of having chosen to pursue information serves as a cue about the importance of that information when it is weighted in subsequent decision making. Festinger (1957) provides evidence that people who perceive conflict between their beliefs and/or actions will change their beliefs to reduce this "cognitive dissonance." Thus, knowledge of having chosen to pursue information encourages higher weighting of the information in subsequent decision making to avoid the dissonance that would occur between low information weights and the choice to pursue information. As noted by Plous (1993), both theories are capable of explaining a variety of findings, and Bastardi and Shafir (1998) and Redelmeier et al. (2001) do not attempt to discriminate between them.

Bastardi and Shafir (1998) do provide evidence against other causal explanations for their findings. For example, much prior research suggests that information is weighed more heavily when it is made more salient, because it demands more of the limited attention that a boundedly rational person can devote to a decision task (Dreben et al. 1979; Duncan 1980).[6] Information pursuit could make information more salient, but Bastardi and Shafir (1998) provide evidence that, holding information salience constant, their results depend on participants making a volitional choice to pursue information.

When applied to the financial statement analysis context, in which financial statements are transformed to treat disclosed information as if it had been recognized, information pursuit is separable into at least three task components that may affect the extent to which disclosed information may affect users' judgments: choice, effort, and reconciliation. A financial statement user with access to disclosed data can choose to exert the effort necessary to compute the financial statement effects of that data and combine those effects with pre-transformation numbers to produce post-transformation numbers. In contrast, a "pure recognition" setting presents post-transformation information, and thus does not include a choice to transform, effortful transformation, or the disaggregation inherent in reconciling pre- and post-transformation numbers. Below we discuss the potential effects of choice, effort, and reconciliation in more detail.

Choice

As noted above, Bastardi and Shafir (1998) and Redelmeier et al. (2001) provide evidence that the choice to receive information affects subsequent information weighting. Financial statement users can make a variety of choices about information pursuit in financial statement analysis, e.g., choosing to transform financial statements to treat disclosed information as if it had been recognized. Standard-setters sometimes make those choices for users by requiring that information be recognized, e.g., changing from allowing only disclosure of stock-compensation expense under SFAS No. 123 to requiring recognition under SFAS No. 123R (FASB 2004). Information pursuit bias suggests that users' act of choosing to transform financial statements affects their subsequent reliance on that information relative to the reliance that would have occurred had the financial statements been transformed to begin with.

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[5] Procedural invariance states that elicitations of preference that are logically equivalent should yield the same preferences (Slovic and Lichtenstein 1983).

[6] For reviews related to attention and salience, see Fischhoff et al. (1978), Taylor and Fiske (1978), Fiske and Taylor (1991), Bloomfield (2002), Hirshleifer and Teoh (2003), and Hirshleifer et al. (2004).

H1: Transformed information affects judgment more when a user has chosen to receive transformed information than when the user has no choice.

Effort

Bastardi and Shafir (1998) and Redelmeier et al. (2001) do not investigate whether effort contributes to information weighting. In the simple vignettes employed in their studies, information pursuit is assumed and information is obtained costlessly and immediately. Yet, the theories that Bastardi and Shafir (1998) and Redelmeier et al. (2001) use to explain effects of choice also suggest a similar effect of effort. Specifically, knowledge of having exerted high effort to obtain information could encourage users to rely on the information because high effort serves as a cue about information importance (cf., Bem 1978) and because reliance on the information avoids cognitive dissonance (cf., Festinger 1957). Prior research in the cognitive-dissonance literature provides further support for this conjecture by demonstrating that people and animals develop more positive attitudes about status or items that are more difficult to obtain (e.g., Aronson and Mills 1959). We extend these theories to predict the effects of effort on the use of information in financial statement analysis. We agree with Maines and McDaniel (2000) and Hirst et al. (2004) that the effortful processing required in many financial statement analysis contexts constitutes a barrier to accessing information, such that many users may not obtain information and therefore will fail to consider that information. However, we suggest that, conditional on users exerting effort to obtain information, they will rely on that information to a greater extent than they would have had the information been obtained with less effort.[7]

H2: Transformed information affects judgment more when effort has been expended to obtain the information.

Reconciliation

Unlike the simple vignettes investigated by Bastardi and Shafir (1998) and Redelmeier et al. (2001), the alternative financial statement treatments examined in our study differ in the extent to which information is reconciled. Specifically, relative to information that is already recognized and aggregated on the face of the financial statements, pursued information is disaggregated and reconciles pre- and post-transformation numbers. This reconciliation provides additional information by indicating the effects of transformation, e.g., indicating the extent to which transformation changes key numbers or ratios, which might provide insight about management's decisions with respect to recognition and disclosure, and therefore management's reporting credibility.[8] Also, reconciliation potentially renders pursued information more salient, so the information may demand more attention in the judgment process. Therefore, we predict that, similar to decision makers in other contexts (e.g., Kahneman 1973; McArthur and Post 1977; MacGregor and Slovic 1986; Payne et al. 1993; Almer et al. 2003), transformed information affects financial statement users' judgments to a greater extent when the effects of the transformation are disaggregated and reconciled, regardless of whether users have chosen to pursue that information or have exerted effort in pursuing that information.

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[7] Staw (1981) provides a review of research on escalation of commitment—the tendency for individuals who have suffered a setback to subsequently commit more resources to the same course of action in an attempt to recoup their losses. Though related to escalation of commitment, our effort prediction is different in that we focus on the weighting of information obtained through effortful pursuit rather than the subsequent resource (or effort) allocations aimed at recouping losses.

> **H3:** Transformed information affects judgment more when it is disaggregated in a reconciliation of pre-and post-transformation information than when it is presented on a post-transformation basis.

Constructive Capitalization of Operating Leases

We investigate our hypotheses in the context of constructive capitalization of operating leases. SFAS No. 13 (FASB 1976) and related authoritative guidance specify thresholds that determine when a leasing arrangement qualifies as a capital lease (with the lessee's balance sheet including the leased asset and lease obligation, along with footnote disclosure of future minimum-lease payments) or an operating lease (with no balance sheet recognition, and the lessee only providing disclosure of future minimum-lease payments). Lessees typically prefer not to capitalize leases, and the precise guidelines in SFAS No. 13 allow various structured leasing arrangements that avoid capitalization (Abdel-khalik 1981; Imhoff et al. 1991). As a result, many leasing arrangements are disclosed in footnotes rather than recognized on balance sheets (SEC 2005).

Standard-setters and regulators have indicated that they believe more leases should be capitalized (e.g., IASC 2000; SEC 2005). Various techniques exist to allow investors to perform calculations that transform the balance sheet to appear as if operating leases had been capitalized (Imhoff et al. 1991; Revsine et al. 2004). These constructive-capitalization techniques are employed by sophisticated users like bond-rating agencies (Houlihan and Sondhi 1984; Standard & Poor's 2005).

Two aspects of the constructive-capitalization setting make it appropriate for testing our hypotheses. First, thorough constructive capitalization involves effortful calculation, as well as knowledge of finance and accounting, and therefore constitutes the sort of transformational financial statement analysis skill that we believe is likely to affect user judgments. Second, testing our hypotheses in this setting allows us to contribute to a current standard-setting debate. Whereas the common rationale for capitalizing operating leases is that it would help those users who do not presently constructively capitalize to attend more to the effects of operating leases, our hypotheses suggest that recognition would ensure that judgments of users who presently constructively capitalize will not be affected by expending the effort to do so.

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[8] It is also possible that reconciliation allows users to better discriminate between pre-transformation numbers and the effects of transformation on the basis of information usefulness, i.e., users might reason that information appearing in footnotes is not recognized because it is less relevant or reliable, and so should be attended to a lesser extent. That response to reconciliation biases against our hypothesis that reconciled information affects judgments to a greater extent.

III. METHOD: EXPERIMENT ONE

Design and Materials

As shown in Figure 1, Experiment One has a 2 × 2 + 1 between-subjects design. In all experimental conditions, participants judge the risk of one company (P. F. Chang's China Bistro, Inc.) relative to that of another (Luby's, Inc.). The experimental materials describe Luby's as having no operating leases and Chang's as having operating leases.[9] Also, in all conditions, participants are provided (or develop) a version of Chang's financial information that treats operating leases as if they had been constructively capitalized. What differs between experimental conditions is whether participants have a choice with respect to considering the effects of constructive capitalization, whether participants must exert effort to perform the constructive capitalization, and the extent to which the effects of constructive capitalization are disaggregated and reconciled.

Specifically, as shown in Figure 2, the benchmark "low choice, low effort, low reconciliation" condition (cell 1 in Figure 1) presents financial information for Chang's as if operating leases have already been constructively capitalized, but the adjustments made to transform the financial information are not shown. Thus, in their judgment of relative risk, participants are asked to compare Luby's numbers (no need to adjust for operating leases because there are none) to Chang's numbers (which have been adjusted to constructively capitalize operating leases).

The four other conditions are all "high reconciliation," in that the effects of constructive capitalization are presented in a disaggregated format that reconciles pre- and post-transformation numbers, and vary in a 2 × 2 design that manipulates "choice" (i.e., whether

FIGURE 1
Experiment One: Design

	Cell 1 LC/LE/LR	 2 LC/LE/HR	 3 LC/HE/HR	 4 HC/LE/HR	 5 HC/HE/HR
Choice[a]	Low	Low	Low	High	High
Effort[b]	Low	Low	High	Low	High
Reconciliation[c]	Low	High	High	High	High

[a] Low-choice (LC) participants were provided all information without choosing to receive (or calculate) it; high-choice (HC) participants received pre-transformation information, but only received (or calculated) the reconciliation of pre- and post-transformation information if they chose to receive (or calculate) it.

[b] Low-effort (LE) participants were provided all financial information; high-effort (HE) participants used the worksheet in Figure 4 to compute amounts necessary to constructively capitalize leases and transform financial statements.

[c] Low-reconciliation (LR) participants were provided post-transformation financial information; high-reconciliation (HR) participants were provided (or obtained) a reconciliation of pre- and post-transformation information.

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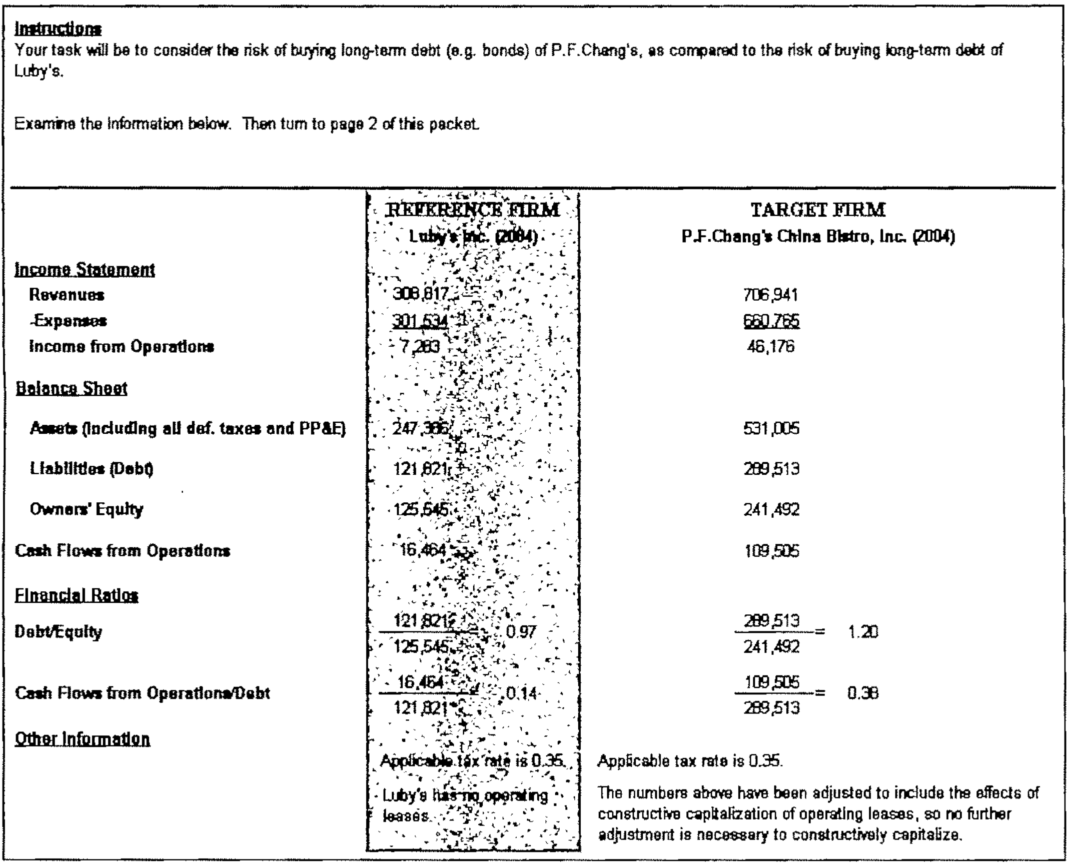
[9] Experimental materials were based on Luby's, Inc. and P. F. Chang's China Bistro, Inc. because these two companies are in the same industry segment but have very different levels of operating leases. Luby's actually has a small amount of operating leases (capitalized leased assets equal to 5.8 percent of total assets), but we transformed their financial statements to constructively capitalize those leases to avoid participants having to consider the effects of constructive capitalization for both firms. Thus, the financial information provided for Luby's appeared as if they have no operating leases, and participants were told that Luby's has no operating leases, such that the only operating leases for participants to consider were Chang's (capitalized leased assets equal to 24.3 percent of total assets).

FIGURE 2
Financial Information Presented in Cell 1 of Experiment One: "Low Choice, Low Effort, Low Reconciliation"



Instructions
Your task will be to consider the risk of buying long-term debt (e.g. bonds) of P.F.Chang's, as compared to the risk of buying long-term debt of Luby's.

Examine the information below. Then turn to page 2 of this packet.

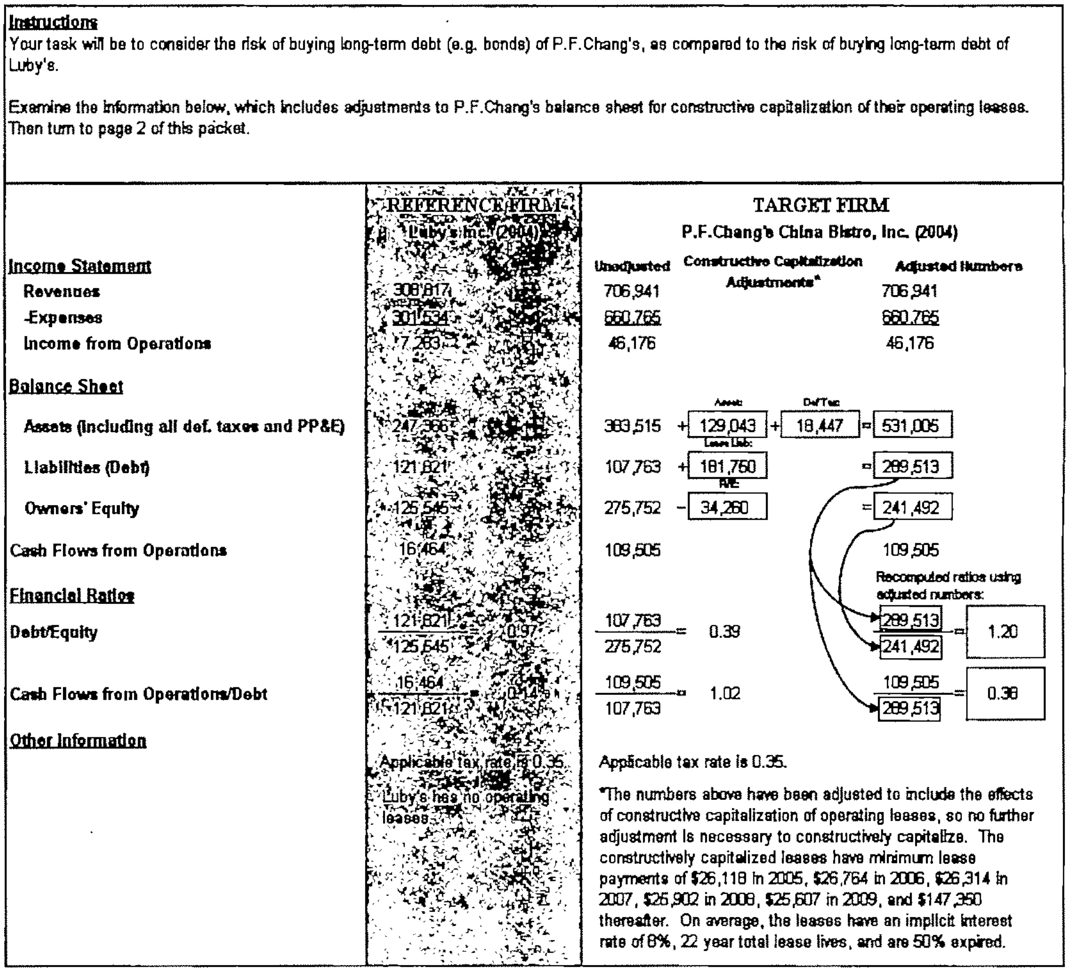
	REFERENCE FIRM Luby's Inc. (2004)	TARGET FIRM P.F.Chang's China Bistro, Inc. (2004)
Income Statement		
Revenues	308,817	706,941
-Expenses	301,534	660,765
Income from Operations	7,283	46,176
Balance Sheet		
Assets (including all def. taxes and PP&E)	247,366	531,005
Liabilities (Debt)	121,821	289,513
Owners' Equity	125,545	241,492
Cash Flows from Operations	16,464	109,505
Financial Ratios		
Debt/Equity	$\frac{121{,}821}{125{,}545} = 0.97$	$\frac{289{,}513}{241{,}492} = 1.20$
Cash Flows from Operations/Debt	$\frac{16{,}464}{121{,}821} = 0.14$	$\frac{109{,}505}{289{,}513} = 0.38$
Other Information	Applicable tax rate is 0.35. Luby's has no operating leases.	Applicable tax rate is 0.35. The numbers above have been adjusted to include the effects of constructive capitalization of operating leases, so no further adjustment is necessary to constructively capitalize.

participants are automatically provided Chang's constructive capitalization or must choose to get that information) and "effort" (i.e., whether participants have to actually perform the calculations necessary to constructively capitalize Chang's operating leases or have those calculations done for them).

The "low choice, low effort, high reconciliation" condition (cell 2 in Figure 1) presents financial information in a disaggregated format in which the effects of constructive capitalization reconcile pre- and post-transformation numbers (see Figure 3). That condition precludes choosing not to constructively capitalize and requires no effort to perform the necessary calculations.

The "high choice, low effort, high reconciliation" condition (cell 4 in Figure 1) presents information in the same disaggregated format as that used in the "low choice, low effort, high reconciliation" condition (cell 2 in Figure 1), but requires participants to choose whether they want to view adjustments for constructive capitalization. These participants initially receive financial information similar to that shown in Figure 3, except that the boxes in Figure 3 are not filled in with numbers, and participants are asked "Do you want

FIGURE 3
Financial Information Presented in Cell 2 of Experiment One: "Low Choice, Low Effort, High Reconciliation"



Instructions

Your task will be to consider the risk of buying long-term debt (e.g. bonds) of P.F.Chang's, as compared to the risk of buying long-term debt of Luby's.

Examine the information below, which includes adjustments to P.F.Chang's balance sheet for constructive capitalization of their operating leases. Then turn to page 2 of this packet.

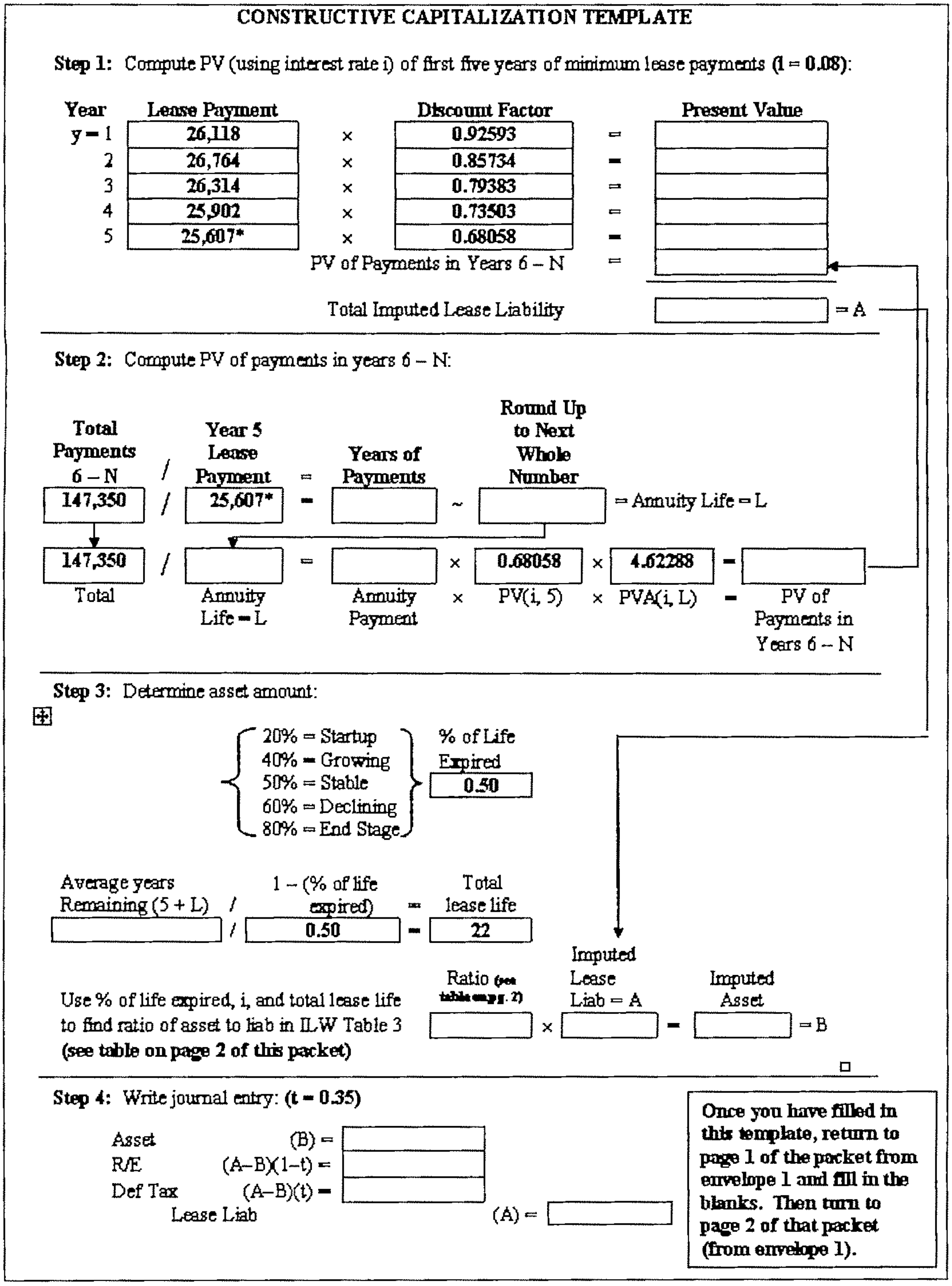
	REFERENCE FIRM Luby's Inc. (2004)	TARGET FIRM P.F.Chang's China Bistro, Inc. (2004) Unadjusted	Constructive Capitalization Adjustments*	Adjusted Numbers
Income Statement				
Revenues	308,817	706,941		706,941
-Expenses	301,534	660,765		660,765
Income from Operations	7,283	46,176		46,176
Balance Sheet				
Assets (including all def. taxes and PP&E)	247,366	383,515	+ Asset: 129,043 + Def/Tax: 18,447	= 531,005
Liabilities (Debt)	121,821	107,763	+ Lease Liab: 181,760	= 289,513
Owners' Equity	125,545	275,752	− R/E: 34,260	= 241,492
Cash Flows from Operations	16,464	109,505		109,505
Financial Ratios				Recomputed ratios using adjusted numbers:
Debt/Equity	121,821 / 125,545 = 0.97	107,763 / 275,752 = 0.39		289,513 / 241,492 = 1.20
Cash Flows from Operations/Debt	16,464 / 121,821 = 0.14	109,505 / 107,763 = 1.02		109,505 / 289,513 = 0.38
Other Information	Applicable tax rate is 0.35. Luby's has no operating leases.	Applicable tax rate is 0.35.		

*The numbers above have been adjusted to include the effects of constructive capitalization of operating leases, so no further adjustment is necessary to constructively capitalize. The constructively capitalized leases have minimum lease payments of $26,118 in 2005, $26,764 in 2006, $26,314 in 2007, $25,902 in 2008, $25,607 in 2009, and $147,350 thereafter. On average, the leases have an implicit interest rate of 8%, 22 year total lease lives, and are 50% expired.

to see adjustments to P. F. Chang's balance sheet for constructive capitalization of their operating leases?" Participants who select "yes" are instructed to open an envelope containing financial information and instructions identical to that shown in Figure 3 (with the previously blank boxes now filled in with numbers). Those who select "no" are directed to continue with their analysis task (i.e., they never see the "filled-in" adjustments and post-transformation numbers).

The "low choice, high effort, high reconciliation" condition (cell 3 in Figure 1) initially presents financial information identical to that provided in the "low choice, low effort, high reconciliation" condition (cell 2 in Figure 1), except that participants in this setting are instructed to use a template (shown in Figure 4) to calculate the effects of constructive

FIGURE 4
Template Used to Calculate Effects of Constructive Capitalization



CONSTRUCTIVE CAPITALIZATION TEMPLATE

Step 1: Compute PV (using interest rate i) of first five years of minimum lease payments (**i = 0.08**):

Year	Lease Payment		Discount Factor		Present Value
y = 1	26,118	×	0.92593	=	
2	26,764	×	0.85734	=	
3	26,314	×	0.79383	=	
4	25,902	×	0.73503	=	
5	25,607*	×	0.68058	=	
	PV of Payments in Years 6 – N			=	
	Total Imputed Lease Liability				= A

Step 2: Compute PV of payments in years 6 – N:

Total Payments 6 – N / **Year 5 Lease Payment** = **Years of Payments** ~ **Round Up to Next Whole Number**

147,350 / 25,607* = ☐ ~ ☐ = Annuity Life = L

147,350 / ☐ = ☐ × 0.68058 × 4.62288 = ☐

Total / Annuity Life = L = Annuity Payment × PV(i, 5) × PVA(i, L) = PV of Payments in Years 6 – N

Step 3: Determine asset amount:

20% = Startup
40% = Growing
50% = Stable
60% = Declining
80% = End Stage

% of Life Expired
0.50

Average years Remaining (5 + L) / 1 – (% of life expired) = Total lease life
☐ / 0.50 = 22

Use % of life expired, i, and total lease life to find ratio of asset to liab in ILW Table 3 **(see table on page 2 of this packet)**

Ratio (see table on pg. 2) × Imputed Lease Liab = A = Imputed Asset
☐ × ☐ = ☐ = B

Step 4: Write journal entry: (**t = 0.35**)

Asset (B) = ☐
R/E (A–B)(1–t) = ☐
Def Tax (A–B)(t) = ☐
Lease Liab (A) = ☐

Once you have filled in this template, return to page 1 of the packet from envelope 1 and fill in the blanks. Then turn to page 2 of that packet (from envelope 1).

capitalization and fill in the blank boxes that appear in the disaggregated reconciliation between pre- and post-transformation financial statements.[10]

The "high choice, high effort, high reconciliation" participants (those in cell 5 in Figure 1) receive the same financial statement information as those in the "low choice, high effort, high reconciliation" condition (cell 3 in Figure 1), but are asked in the instructions "Do you want to adjust P. F. Chang's balance sheet for constructive capitalization of their operating leases?" Those who select "yes" are instructed to open an envelope containing financial information (and instructions) identical to that given to "low choice, high effort, high reconciliation" participants. Those who select "no" are directed to continue with their analysis task (i.e., they never see the "filled-in" adjustments and post-transformation numbers).

In this design, comparisons between the benchmark "low choice, low effort, low reconciliation" condition and the "low choice, low effort, high reconciliation" condition reveal effects of reconciliation. Holding reconciliation constant at "high," comparisons between the low- and high-choice conditions reveal effects of choice, and comparisons between the low- and high-effort conditions reveal effects of effort.[11] The benchmark cell captures a "pure recognition" setting in which all operating leases are constructively capitalized (which has been proposed but does not exist in practice), while the "high choice, high effort, high reconciliation" cell captures the situation faced currently by an investor who could choose to constructively capitalize operating leases. Other cells likewise capture situations that could be encountered in practice.

Participants

Testing our hypotheses requires participants who are very familiar with a financial statement analysis skill that could be used to transform financial statements to appear as if operating leases had been capitalized. Therefore, we use as participants 108 students enrolled during Spring 2005 in an M.B.A.-level intermediate accounting course at Cornell University's Johnson Graduate School of Management. Prior to data collection the students had all participated in class sessions that dealt with capital leases and operating leases, and had gained experience with constructive capitalization using a transformation template that provides an outline for using footnote disclosures of operating lease obligations to calculate and place implied assets and liabilities on the balance sheet of the lessee. One hundred participants provided data during an optional in-class exercise in three successive sections of the same accounting course; eight participants missed the in-class session and so provided data in arranged sessions outside of class.[12] On average, participants had 5.2 years of work experience, were 28.8 years old, and had taken or were currently taking 5.8 finance and accounting classes. Seventy-three percent of the participants were male.

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[10] All information (other than present value factors and intermediate calculations) provided in the template shown in Figure 4 was also provided in the footnote and financial information provided in all high-reconciliation cells (see Figure 3) to reduce the potential for an information confound. As discussed in Section V, Experiment Two provides low-effort participants with the completed template to address more directly the potential for an information confound.

[11] Choice and effort were not manipulated within the "low reconciliation" setting, because both "high choice" and "high effort" in our experiment lead to the presentation of information in its reconciled format ("high reconciliation").

[12] Participants were randomly assigned to treatment cells within sessions. Dropping participants who provided data outside of class does not alter implications of any analysis, and session is not significant in any analysis, so these variables are not discussed further.

Dependent Variable

Participants answered the following question: "Please rate the risk of buying long-term debt (e.g., bonds) of P. F. Chang's, as compared to the risk of buying long-term debt of Luby's, on a scale of 0 to 100." On the scale, 0 was described as "P. F. Chang's is much less risky than Luby's," 50 as "P. F. Chang's is about the same risk as Luby's," and 100 as "P. F. Chang's is much more risky than Luby's." We focused on assessments of the risk of buying debt because constructive capitalization of operating leases affects primarily the balance sheet (Imhoff et al. 1991), and prior research suggests that balance sheet numbers play an important role in these assessments.[13] We used a relative rather than absolute risk assessment to increase power by decreasing noise that could result from participants' idiosyncratic interpretation of the response scale. We avoid making predications as to the levels of the relative risk rating selected by participants as there are likely a number of non-lease-related reasons that P. F. Chang's may be seen as more or less risky than Luby's on average (e.g., the names of the two restaurant chains, familiarity with the chains, the fact that Chang's is larger and has more cash flow than Luby's, etc.). However, by holding constant all of Luby's information and manipulating only P. F. Chang's financials, we are able to address differences in relative risk ratings between cells.

Independent Variables

Choice is manipulated by varying whether participants can choose whether to receive (or calculate) information about the effects of constructive capitalization. High-choice participants can choose whether to receive (or calculate) that information; low-choice participants are provided that information (or are instructed to calculate it). We anticipated that virtually all participants would choose to receive (or calculate) this information. The choice variable is not intended to manipulate the information available to participants, but rather to manipulate the extent to which participants attribute their possession of that information to their own decision to obtain it.[14]

Effort is manipulated by varying whether participants perform the calculations necessary to generate post-transformation financial statements prior to making their risk assessment. Bonner and Sprinkle (2002) describe effort as involving four attributes: direction, duration, intensity, and strategy development. Our effort manipulation varies duration and intensity. High-effort participants are provided the template shown in Figure 4 and perform the calculations necessary to constructively capitalize operating leases and "fill in the blanks" necessary to transform financial information prior to making their risk assessment. This activity involves many calculations, and therefore should be relatively intense and increase the duration of the analysis task. Low-effort participants are not provided the template shown in Figure 4, and instead are provided all of the information they use for their risk assessment (see Figure 3), so they do not have to engage in the laborious calculations performed by high-effort participants.

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[13] For example, see Wilcox (1971), Abdel-khalik et al. (1978), Wilkins and Zimmer (1983a), Harper et al. (1987), and Harper and Mister (1991).

[14] If participants do not choose to acquire the leasing information, they will have fundamentally different information as compared to participants who acquire or are provided that information, such that information and choice to pursue information are confounded in analyses. Also, participants in the "high choice, high effort" condition who do not choose to acquire the information do not exert high effort, thus weakening the effort manipulation. By operationalizing a choice condition in which most participants are likely to choose to pursue information, we avoid these problems, but may have a weaker choice treatment than we would have if participants viewed their choice as more controversial.

Reconciliation is manipulated by varying whether participants ever see the effects of constructive capitalization disaggregated via a reconciliation of pre- and post-transformation financial statements. High-reconciliation participants receive (or generate, if in a high-effort condition) a reconciliation of pre- and post-transformation information; low-reconciliation participants receive only post-transformation financial information (see Figure 2).[15]

Procedure

All participants began the experiment by completing an informed-consent, human-subjects-approval form. They then read a short set of instructions and examined financial information for two companies: Luby's, Inc., portrayed as having no operating leases, and P. F. Chang's China Bistro, Inc., which has operating leases. The financial information included information from the income statement (revenues, expenses, and income from operations), balance sheet (assets, liabilities, and owners' equity), cash flow from operations, debt/equity ratio, cash flow from operations/debt, the applicable tax rate of 35 percent, and information about operating leases.[16] Participants in the low-effort, low-choice cells 1 and 2 were provided post-transformation information that included the effects of Chang's constructive capitalization; participants in the other cells were provided pre-transformation information.

Participants in high-choice conditions next determined whether they wanted to receive (cell 4) or calculate (cell 5) the effects of constructive capitalization, and participants in high-effort conditions (cells 3 and 5) calculated the effects of constructive capitalization. Calculation required using a template based on Imhoff et al. (1991) (see Figure 4). As shown in Figure 3, updating the financial information to reflect the results of constructive capitalization affects Chang's PP&E, deferred taxes, lease liability, and retained earnings, and therefore increases Chang's debt/equity ratio and decreases the ratio of cash flows from operations to debt in such a way as to increase Chang's leverage and decrease its apparent ability to service debt.[17] These changes indicate increased risk of Chang's; thus, considering this information should lead to higher risk assessments.

All participants then made their risk assessment, answered several debriefing questions, and sealed that information in an envelope. Next, all participants who had not already calculated the effects of constructive capitalization (i.e., those in the low-effort conditions and any participants who chose not to perform the calculations in the "high choice, high effort" condition) did so to provide data for a knowledge check. All participants concluded the experiment by answering a short demographic questionnaire.

IV. RESULTS: EXPERIMENT ONE

Two participants' responses to a comprehension check indicated that they did not understand whether they were working with financial information that was transformed to reflect constructive capitalization. Data from those participants are omitted from all hypotheses tests, leaving 106 observations. Inferences are unchanged if the two participants' data are included.

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[15] As can be seen from comparing Figures 2 and 3, the footnote provided to low-reconciliation participants did not include the information about minimum lease payments, interest rates, etc., that would be necessary to reconstruct the constructive capitalization calculations that were used to calculate post-transformation information. We made this design choice to make sure that low-reconciliation participants could not perform the constructive capitalization and convert their setting into the high-choice, high-effort, high-reconciliation condition.

[16] The debt/equity ratio and the ratio of cash flows from operations to debt were given because these ratios are among the most commonly used in debt rating (Zimmer 1979; Abdel-khalik and El-Sheshai 1980; Dietrich and Kaplan 1982; Palepu et al. 2000; Standard & Poor's 2005).

[17] To be consistent with participants' course work, which had focused on balance sheet effects of constructive capitalization, the financial information provided to participants did not indicate any effect of constructive capitalization on income from operations or cash flow from operations.

Table 1, Panel A shows mean and mean-ranked risk ratings, by cell, and Table 1, Panel B shows results of planned comparisons. Cell variances are not equal, so hypotheses tests are based on nonparametric analyses of rank-transformed data.[18] Inferences are unchanged when based on parametric tests.

Hypothesis Tests

Hypothesis 1 predicts that, holding reconciliation constant at "high," transformed information affects judgment more when a user has chosen to transform the information. To test H1, we compare risk assessments between high- and low-choice conditions in the high-reconciliation cells (cells 2–5). Results indicate an insignificant effect of choice (means of 59.0 for low choice and 55.6 for high choice; $t = -0.55$; ns).[19] These results do not support H1.

Hypothesis 2 predicts that, holding reconciliation constant at "high," transformed information affects judgment more when effort has been expended to obtain that information. To test H2, we compare risk assessments between high- and low-effort conditions in the high-reconciliation cells (cells 2–5). Results indicate a significant effect of effort (means of 51.4 for low effort and 62.9 for high effort; $t = 1.83$; $p = 0.035$).[20,21] These results support H2.[22]

Our design controls for or enables tests of alternative explanations for the effort effect. One explanation is that effort might increase the salience of constructive-capitalization information. However, as shown in Figure 3, the results of constructive capitalization were highly salient regardless of effort, so this explanation appears unlikely. Another possible explanation for the effort effect is that participants in the high-effort conditions were reacting to resolution of uncertainty, since high-effort participants were first presented with financial information that highlighted missing items and then resolved uncertainty about those items by calculating them to produce post-transformation numbers. Our experimental design allows us to test this explanation by comparing risk ratings between participants in the "high choice, low effort" cell (who, like high-effort participants, were first presented with financial information that highlighted missing items and then resolved uncertainty by choosing to receive a completed reconciliation) and the "low choice, low effort" cell (who

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[18] Ott and Longnecker (2001) suggest using Levene's Test to test for homogeneity of cell variances. Results indicate that cell variances are not equal ($F = 2.64$, $p = 0.038$).

[19] If participants chose not to obtain the information associated with constructive capitalization, then interpretation of the choice variable would be difficult, as differences between choice conditions could be attributed to high-choice participants not considering the effects of constructive capitalization, as well as to the psychological effect of choice on information weighting. However, as anticipated, almost all participants chose to obtain the constructive-capitalization information. Inferences are unchanged when the three subjects who chose not to obtain that information are dropped from analyses.

[20] Because high-effort participants judge risk based on financial information that they themselves transformed, mistakes in their calculations could affect their risk ratings. However, inferences are unchanged if we drop participants who made mistakes. Specifically, inferences are unchanged if we drop the 11 participants in cells 3 and 5 who made mistakes in their transformation calculations such that their final ratio calculations were off by more than 1 percent. Further, inferences are unchanged if (for consistency) we drop 15 participants in cells 1, 2, and 4 who did the transformation post-experimentally as part of their debriefing and made mistakes in their transformation calculations that rendered their final ratio calculations off by more than 1 percent.

[21] All p-values presented in the paper are one-sided when directional effects are hypothesized.

[22] We do not hypothesize an interaction between effort and choice, and no significant interaction is found in any analyses.

TABLE 1
Experiment One: Risk Ratings[a] and Hypothesis Tests

Panel A: Cell Number, n, and Mean Rank,[b] Risk Ratings (Mean) [Median] {Std. Dev.}

Low Reconciliation				High Reconciliation			
	Effort				Effort		
Choice	**Low**	**High**	**All**	**Choice**	**Low**	**High**	**All**
Low	Cell 1 n = 19 36.4 (29.7) [25.5] {16.0}			Low	Cell 2 n = 21 51.8 (42.3) [30.0] {26.7}	Cell 3 n = 21 66.2 (49.7) [50.5] {24.3}	59.0 (46.0) [39.8] {25.5}
High				High	Cell 4 n = 23 51.0 (38.0) [31.0] {17.0}	Cell 5 n = 22 59.9 (46.0) [50.0] {24.3}	55.6 (42.1) [35.0] {23.6}
All				All	51.4 (40.1) [31.0] {22.1}	62.9 (47.8) [50.0] {26.3}	57.2 (44.0) [37.5] {24.5}

Panel B: Results of Planned Comparisons[c]

Planned Comparisons[d]	Estimate	t	p-value (one-sided)
H1: Choice: Cells 2 and 3 (LC) versus Cells 4 and 5 (HC)	−3.5	−0.55	ns
H2: Effort: Cells 2 and 4 (LE) versus Cells 3 and 5 (HE)	11.7	1.83	0.035
H3: Reconciliation: Cell 1 (LR/LE/LC) versus Cell 2 (HR/LE/LC)	15.3	1.63	0.053

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[a] Participants rated the risk of buying long-term debt (e.g., bonds) of P. F. Chang's, as compared to the risk of buying long-term debt of Luby's, on a scale of 0 to 100. On the scale, 0 was described as "P. F. Chang's is much less risky than Luby's," 50 as "P. F. Chang's is about the same risk as Luby's," and 100 as "P. F. Chang's is much more risky than Luby's."

[b] With 106 subjects making risk-rating decisions, rank can vary from 1 to 106, while the range for the risk rating mean is 0 to 100.

[c] p-values are associated with nonparametric analyses of rank-transformed data. Inferences are unchanged when based on parametric analyses.

[d] LC = low choice, HC = high choice, LE = low effort, HE = high effort, LR = low reconciliation, HR = high reconciliation.

were provided the completed reconciliation to begin with, and so did not face uncertainty about highlighted numbers). That comparison indicates no difference (see cells 4 and 2 in Table 1), and therefore indicates that resolution of uncertainty alone does not explain the effort effect.

Hypothesis 3 predicts that transformed information affects judgment more when it is displayed in a disaggregated format that reconciles pre- and post-transformation numbers. To test H3, we compare risk assessments between cells 1 and 2, holding effort and choice constant at "low" and varying reconciliation. Results of that comparison indicate a significant effect of reconciliation (means of 36.4 for "low choice, low effort, low reconciliation" and 51.8 for "low choice, low effort, high reconciliation"; t = 1.63; p = 0.053). These results support H3.

Additional Analyses

Immediately following their risk-rating decision, participants answered three questions during debriefing that provide further insight into their risk ratings. Descriptive data are provided in Table 2. Because assumptions for parametric analyses are satisfied, debriefing data are analyzed using parametric planned comparisons.

First, participants rated the effort they expended prior to making their risk rating. Focusing on the high-reconciliation conditions, participants in the high-effort conditions (cells

TABLE 2
Experiment One: Debriefing Data

Panel A: Means [Medians] {Std. Dev.}

	Cell[a]				
Debriefing Variable	**1: LC/LE/LR**	**2: LC/LE/HR**	**3: LC/HE/HR**	**4: HC/LE/HR**	**5: HC/HE/HR**
Perceived	47.7	46.9	53.1	48.8	57.8
Effort	[50.0]	[50.0]	[50.0]	[50.0]	[60.0]
Ratings[b]	{20.7}	{22.3}	{21.9}	{20.1}	{24.7}
Management	53.4	40.5	45.2	45.4	46.8
Credibility	[50.0]	[50.0]	[50.0]	[50.0]	[50.0]
Ratings[c]	{7.5}	{12.6}	{19.3}	{17.2}	{17.4}
Perceived	49.9	59.2	65.7	58.8	75.2
Informedness	[50.0]	[60.0]	[70.0]	[69.5]	[80.0]
Ratings[d]	{17.1}	{22.5}	{21.4}	{21.2}	{19.1}

Panel B: Significant Planned Comparisons

	Estimate	**t**	**p-value (one-sided)**
Does Effort Affect Perceived Effort?	7.6	1.60	0.056
Cells 2 and 4 (LE) versus Cells 3 and 5 (HE)			
Does Reconciliation Affect Perceived Mgmt. Credibility?	−12.9	−2.57	0.006
Cell 1 (LC/LE/LR) versus Cell 2 (LC/LE/HR)			
Does Reconciliation Affect Perceived Informedness?	9.3	1.44	0.077
Cell 1 (LC/LE/LR) versus Cell 2 (LC/LE/HR)			
Does Effort Affect Perceived Informedness?	11.4	2.61	0.005
Cells 2 and 4 (LE) versus Cells 3 and 5 (HE)			

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[a] LC = low choice, HC = high choice, LE = low effort, HE = high effort, LR = low reconciliation, HR = high reconciliation.

[b] Participants rated the effort they expended on analysis relevant to their risk rating on a scale of 0 to 100. On the scale, 0 was described as "little effort," 50 as "some effort," and 100 as "much effort."

[c] Participants rated the credibility of the management of P. F. Chang's, as compared to the credibility of the management of Luby's, on a scale of 0 to 100. On the scale, 0 was described as "P. F. Chang's is much less credible than Luby's," 50 as "P. F. Chang's is about the same credibility as Luby's," and 100 as "P. F. Chang's is much more credible than Luby's."

[d] Participants rated the extent to which they believe they are more (or less) informed than an average investor with respect to P. F. Chang's on a scale of 0 to 100. On the scale, 0 was described as "I'm much less informed," 50 as "I'm about equally informed," and 100 as "I'm much more informed."

3 and 5) rated their effort as higher than participants in the low-effort conditions (cells 2 and 4) (means of 55.5 and 47.9, respectively; t = 1.60; p = 0.056). This result suggests both that our manipulation successfully induced participants in the high-effort setting to exert more effort and that they were aware that they were exerting high effort. Perceived effort is not significantly associated with risk ratings, so effort perceptions do not play a mediating role. No other comparison or interaction is significant in analyses that used perceived effort as a dependent variable.

Participants also rated the credibility of P. F. Chang's management relative to that of Luby's management. Focusing on only cells 1 and 2, which hold constant low effort and low choice but vary reconciliation, participants rated P. F. Chang's management as relatively less credible in the high-reconciliation condition than in the low-reconciliation condition (means of 40.5 and 53.4, respectively; t = −2.57; p = 0.006). Perceived credibility is not significantly associated with risk ratings, so credibility perceptions do not play a mediating role. No other comparisons or interactions were significant in analyses that used perceived management credibility as a dependent variable.

Participants also rated the extent to which they believe they are more informed than is the average investor. Focusing on only cells 1 and 2, which hold constant low effort and low choice but vary reconciliation, participants believed they were more informed in the high-reconciliation condition (means of 49.9 for low reconciliation and 59.2 for high reconciliation; t = 1.44; p = 0.077). Focusing on cells 2–5, which hold reconciliation constant at high and vary choice and effort, participants believed they were more informed in the high-effort conditions (means of 60.0 for low effort and 70.6 for high effort; t = 2.61; p = 0.005), but not in the high-choice conditions (means of 62.5 for low choice and 67.2 for high choice; t = 1.03; ns). No other comparisons or interactions were significant in analyses that used perceived informedness as a dependant variable.

Additional analyses suggest that perceived informedness may partially mediate the effect of effort and reconciliation on risk ratings. The framework described by Baron and Kenny (1986) to test for mediation requires the following conditions to hold: (1) a significant relation between the independent variable (effort or reconciliation) and the dependent variable (ranked risk ratings), (2) a significant relation between the independent variable and the mediator (perceived informedness), (3) a significant relation between the mediator and the dependent variable, and (4) a decline in the significance of the relation between the independent variable and the dependent variable when the mediator is included in the model. Condition 1 is supported by our hypothesis tests. Analyses (not shown) support conditions 2 and 3, and also support condition 4 by indicating a decline in the significance of effort (from p = 0.035 to p = 0.081) and reconciliation (from p = 0.053 to p = 0.081) when perceived informedness is also included in the model, suggesting that perceived informedness partially mediates the effect of effort and reconciliation on risk ratings.

V. EXPERIMENT TWO

Overview

Experiment Two is designed to further test the effort effect identified in the high-reconciliation conditions of Experiment One. Recall that low-effort participants in Experiment One were not provided a completed constructive capitalization template (see Figure 4), but rather were provided the results of those calculations in a completed reconciliation of pre- and post-transformation numbers (i.e., the information in Figure 3). This design choice made it possible for low-effort participants to complete the template later in Experiment One to supply data for a knowledge check (providing participants with the template early in the experiment would have invalidated the knowledge check). However, this choice leaves open the possibility that differences in risk ratings that we attributed to effort could instead be attributed to access to the present value factors or calculations included in the template. Therefore, Experiment Two replicates the effort effect while holding constant access to all information contained in the constructive-capitalization template.

Method

Except as noted otherwise, the method in Experiment Two is the same as that used in Experiment One.

Design, Materials, and Independent Variable

Experiment Two has a 1 × 2 between-subjects design, varying effort. High-effort participants perform the calculations necessary to constructively capitalize operating leases and "fill in the blanks" necessary to transform financial information prior to making their risk assessment. Low-effort participants are provided versions of those materials with the blanks already filled in.

Participants

Participants were 104 students enrolled during Spring 2006 in an M.B.A.-level intermediate accounting course at Cornell University's Johnson Graduate School of Management.[23] As in Experiment One, prior to data collection the students had all participated in class sessions that dealt with capital leases and operating leases, and had gained experience with constructive capitalization using a transformation template that provides an outline for using footnote disclosures of operating lease obligations to calculate implied assets and liabilities and place them on the balance sheet of the lessee. Participants provided data during an optional in-class exercise in three successive sections of the same accounting course. On average, participants had 5.0 years of work experience, were 28.7 years old, and had taken or were currently taking 6.3 finance and accounting classes. 80 percent of the participants were male.

Procedure

The procedure was the same as in Experiment One, except that the debriefing questions started with a request that participants write in a blank the time shown on a digital clock displayed in the classroom. The experimenters recorded the time when participants began the experiment, so the difference in recorded time could be used to provide a manipulation check by determining whether time spent on the risk analysis task differed between treatment conditions.

Results

Table 3, Panel A shows descriptive statistics; Table 3, Panel B shows results of planned comparisons. Because assumptions for parametric analyses are satisfied, data are analyzed using parametric planned comparisons. Inferences are unchanged when based on nonparametric tests.

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[23] One hundred eleven participants were initially recruited, but seven "high effort" observations were unusable because participants failed to provide risk ratings. Two of these seven participants may have either run out of time or opted not to complete the study after having begun, because they provided no data beyond their constructive capitalization calculations. The other five participants provided debriefing data, suggesting they may have simply overlooked the dependent measure questions after completing the constructive capitalization task.

TABLE 3
Experiment Two: Results

Panel A: Means [Medians] {Std. Dev.} and Significant Planned Comparisons

Variable	Cell Low Effort (n = 56)	 High Effort (n = 48)
Risk Rating[a]	43.9 [40.0] {21.5}	53.2 [51.0] {25.7}
Time[b]	7.1 [7.0] {2.5}	13.3 [13.0] {2.2}
Perceived Effort Ratings[c]	46.3 [49.0] {18.4}	50.9 [50.0] {24.8}
Management Credibility Ratings[d]	46.4 [49.0] {19.5}	42.2 [40.5] {20.5}
Perceived Informedness Ratings[e]	61.9 [64.0] {21.3}	68.1 [70.0] {21.1}

Panel B: Planned Comparisons

	Estimate	t	p-value (one-sided)
Does Effort Affect Risk Rating?	9.2	2.00	0.024
Does Effort Affect Time Spent?	6.2	13.44	<0.001
Does Effort Affect Perceived Effort?	4.5	1.06	ns
Does Effort Affect Perceived Management Credibility?	−4.0	−1.04	ns
Does Effort Affect Perceived Informedness?	6.2	1.48	0.071

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[a] See Table 1, Note a.
[b] "Time" is the minutes elapsed between the beginning of the experiment and the beginning of debriefing. During this interval, participants read instructions, examined information provided for Luby's and P. F. Chang's, examined (or performed) the calculations necessary to constructively capitalize operating leases and include their effects in the financial statements, and rated risk, effort, informedness, and management credibility.
[c] See Table 2, Note b.
[d] See Table 2, Note c.
[e] See Table 2, Note d.

Manipulation Check

The first debriefing question asked participants to record the time shown on a digital clock displayed in the room where they provided data. Table 3 shows that the mean time elapsed between the beginning of the experiment and the start of debriefing was 13.3 minutes for participants in the high-effort condition, which was significantly higher than the 7.1 minutes elapsed for participants in the low-effort condition ($t = 13.44$; $p < 0.001$). Recall that duration is one of the dimensions of effort identified by Bonner and Sprinkle (2002). This data suggest that the effort treatment manipulated duration successfully.

Hypothesis Test

Experiment Two focuses on H2, which predicts that, holding reconciliation constant at "high" and choice constant at "low," transformed information affects judgment more when effort has been expended to obtain that information. To test H2, we compare risk assessments between high- and low-effort conditions. Results indicate a significant effect of effort (means of 43.9 for low effort and 53.2 for high effort; $t = 2.00$; $p = 0.024$). These results support H2. Thus, even when low-effort participants have access to all the calculations underlying constructive capitalization, that information affects their judgments less than it does for high-effort participants who have to perform the calculations themselves.[24]

Additional Analyses

As in Experiment One, participants in Experiment Two answered three questions immediately following their risk-rating decision. First, participants rated the effort they expended prior to making their risk rating. Unlike Experiment One, perceived effort did not differ between participants in the high- and low-effort conditions (means of 50.9 and 46.3, respectively; $t = 1.06$; $p = \text{ns}$). Given that high-effort participants in Experiments One and Two had the same task, this result suggests that provision of the completed template to low-effort participants in Experiment Two might have encouraged them to examine the template enough to perceive their effort to be relatively high.

Participants also rated the credibility of P. F. Chang's management relative to that of Luby's management. As in Experiment One, effort did not significantly affect participants' credibility assessments (means of 46.4 for low effort and 42.2 for high effort; $t = -1.04$; ns).

Participants also rated the extent to which they believe they are more informed than is the average investor. High-effort participants' informedness ratings are marginally significantly greater than those of low-effort participants (means of 68.1 and 61.9, respectively; $t = 1.48$; $p = 0.071$), suggesting that, like participants in Experiment One, these participants believed they were somewhat more informed when they had exerted higher effort. As in Experiment One, analyses (not shown) suggest that perceived informedness may partially mediate the effect of effort on risk ratings, indicating a decline in the significance of effort (from $p = 0.033$ to $p = 0.066$) when perceived informedness is also included in the model.[25]

VI. CONCLUSION

This paper reports the results of two experiments that examine how financial statement users' judgments are influenced by the process of transforming financial statements to appear as if disclosed information had been recognized. Holding constant that users had access to financial statements that were transformed to constructively capitalize operating leases, users' risk assessments were influenced more by the effects of operating leases when those effects were reconciled with pre-transformation numbers and when users had exerted the effort necessary to calculate those effects and transform the financial statements.

These results have implications for literatures investigating format effects, fundamental analysis, decision aids, and basic psychological processes. We provide evidence that disclosed information that is acquired via reconciled information displays and/or effortful processing influences financial decision making to a greater extent than it would have otherwise. Therefore, the process by which fundamental analysis occurs and by which decisions are aided in that analysis can influence the extent to which the results of that analysis affect decisions. For example, our results suggest that requiring financial statement users to perform their own constructive capitalization will result in that information affecting their risk assessments to a greater extent than would be the case had they been provided with financial statements that were already transformed. More generally, conditional on a user transforming financial statements to treat disclosed information as if it had been recognized, the user might rely on that information to a greater extent than he would have had the information been recognized to begin with.

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[24] Given that Bonner and Sprinkle (2002) identify duration as a dimension of effort, we tested whether the time measure used for a manipulation check mediates the effect of effort on risk rating. Time does not predict risk rating ($t = -0.04$, ns), so mediation is not possible (Baron and Kenny 1986).

[25] One participant did not provide an informedness rating, and thus should not be used in any portion of the mediation analysis; thus, the significance of the effort effect in this analysis (when not controlling for informedness) is $p = 0.033$, rather than $p = 0.024$ as reported on Table 3.

The significant effort and reconciliation effects imply that judgments close to a pure recognition setting could be obtained by a user delegating transformational procedures to someone else and subsequently working only with the post-transformation statements. However, it is also important to note that participants in our experiments believed that reconciliation provided information about management credibility. Just as Elliott (2006) demonstrates that reconciliation helps less sophisticated users back out the effects of adjustments used to generate pro forma earnings, we demonstrate that reconciliation helps users to consider the relatively large amounts of off-balance-sheet liabilities that transformation can bring onto the balance sheet. Therefore, to the extent a user wants to consume the information inherent in reconciliation, it might be preferable to delegate only the effortful calculation necessary for preparation of the reconciliation.

While our experiments are set in the leasing context, there exist numerous other financial statement analysis contexts in which users potentially transform financial statements, e.g., dealing with differences in accounting for stock compensation, inventory cost flows, pensions and health care benefits, revenue recognition, and asset securitization and derecognition. Analysts also perform transformations that remove transitory elements from income, or that generate or modify other pro forma financial statements. Some of these contexts naturally involve less effortful calculations (e.g., stock compensation), while others might involve more effortful use of knowledge and calculation (e.g., pensions). A useful direction for future research is to seek to generalize our results to other contexts.

We find no support for the finding by prior psychology research (Bastardi and Shafir 1998; Redelmeier et al. 2001) that the effects of information pursuit depend on a decision maker having chosen to pursue the information. However, it is important to note that this lack of result could be due to low power of our choice treatment. Participants may have viewed using transformed data as obviously necessary to be able to provide an accurate risk assessment, and so perceived no real choice being made. In fact, we depended on virtually all participants choosing to obtain information about constructive capitalization so that we could hold constant that all participants possessed post-transformation information when making their risk assessments. Though many financial statement analysis choices are likely similarly straightforward, future research could examine whether the effects of choice are stronger when participants are choosing among approaches that are more controversial.

Future research could also seek to measure effort more precisely and to discriminate further between alternative explanations for the effect of effort that we observe. Regarding effort measurement, Bonner and Sprinkle (2002) suggest that direction, duration, intensity, and coping-strategy formation are all dimensions of effort. Our manipulation of effort was designed to affect duration and intensity via the laborious calculations necessary to constructively capitalize leases. Consistent with this explanation, our debriefing data from Experiment Two indicate that high-effort participants exerted effort for longer duration. However, our paper-and-pencil task precluded us from using the precise measurement of duration and intensity that might be possible in a computerized data-collection methodology.

Regarding alternative explanations for effort effects, one surprising result is that data from both experiments indicate that perceived effort does not mediate the effect of effort on risk ratings. This result could be due to a somewhat noisy perceived-effort measure, but could also indicate that the effect of effort on risk ratings is less due to perceived effort providing a cue of importance or creating dissonance and more because exerting effort results in deeper processing or encourages particular inferences. Consistent with this view, debriefing data from both experiments indicate that participants believed they were better informed in the high-effort conditions (regardless of their perceived effort), and this "perceived informedness" partially mediates the effect of effort on risk ratings. Furthermore, although the high salience of lease information in both conditions of Experiment Two suggests that the effect of effort on judgment does not occur because of differential salience of display, it still may be the case that effort increases information salience incrementally beyond that provided by display. As discussed previously, Experiment One's design does allow us to preclude the alternative explanation that effort affects judgment because it resolves uncertainty, and Experiment Two allows us to preclude the alternative explanation that effort affects judgment because high-effort participants have access to different information. A useful direction for future work would be to examine further the mechanisms by which effort affects judgment.

Future research could also generalize our results to other groups of financial decision makers and practice contexts. Our experiments use finance-oriented M.B.A. students to proxy for financial statement users, because these participants had recent, homogeneous experience about a financial statement analysis technique (constructive capitalization of operating leases) that could be used to transform financial statements to appear as if disclosed information had been recognized. Elliott et al. (2007) provide evidence that such "select" M.B.A. students are good proxies for nonprofessional investors, but our participants may also proxy for professional users, given that they are employing a relatively sophisticated transformational analysis skill. Future research could seek to generalize this result to more experienced analysts and to various practice settings, considering how variables present in such settings (e.g., incentives, time pressure, etc.) might moderate the effects we observe.

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THE ACCOUNTING REVIEW
Vol. 82, No. 3
2007
pp. 759–796

The Effects of Contracting, Litigation, Regulation, and Tax Costs on Conditional and Unconditional Conservatism: Cross-Sectional Evidence at the Firm Level

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ABSTRACT: Prior studies propose four explanations for accounting conservatism, and recent studies classify conservatism into two forms: conditional and unconditional conservatism. This paper examines whether each proposed explanation applies to conditional conservatism, unconditional conservatism, or both. The findings are as follows: (1) contracting induces conditional conservatism; (2) litigation induces both forms; (3) regulation induces unconditional conservatism; and (4) taxation induces unconditional conservatism. These findings indicate that the two forms of conservatism play distinct roles in contracting, regulation, and taxation, as well as a common role in litigation. They also play an interrelated role, as suggested by the finding that unconditional conservatism reduces conditional conservatism. The combined evidence implies that because the two forms meet distinct needs but are negatively interrelated, it is necessary to trade them off.

Keywords: *conditional conservatism; unconditional conservatism; contracting; taxation.*

Data Availability: *All data are available from public sources.*

I. INTRODUCTION

Researchers have proposed four explanations for accounting conservatism: *contracting*, *litigation*, *regulation*, and *taxation* (Watts 2003). The contracting explanation suggests that conservatism counteracts firms' incentives for aggressive accounting and, hence, lowers potential losses to investors who use accounting numbers in contracts. To induce conservatism for contracting purposes, the investors can impose on firms certain costs (hereafter, contracting costs). The litigation explanation points out that firms also face asymmetric litigation costs that induce conservatism. Under the regulation hypothesis, accounting standard-setters and regulators prefer conservatism and induce it by imposing

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This paper is based on my dissertation at the State University of New York at Buffalo. I appreciate the helpful comments and suggestions from my dissertation committee—Jerry Han (former Chair, now deceased), Michael Rozeff (Chair), Samuel Tiras (Chair), and Susan Hamlen—as well as those from two anonymous referees, Dan Dhaliwal (editor), Sudipta Basu (the conference discussant). The paper also benefited from the comments of Gary Fleischman, Frederic Sterbenz, Steven Rock, Penne Ainsworth, Linda Kidwell, Phil Shane, John Jacob, and other workshop participants at the University of Hong Kong, State University of New York at Buffalo, University of Wyoming, and University of Colorado, and the 2005 AAA Annual Meeting.

Editor's note: This paper was accepted by Dan Dhaliwal.

Submitted October 2005
Accepted September 2006

regulation costs on firms. To lower the contracting, litigation, and regulation costs, firms tend to report conservatively. Finally, the taxation hypothesis posits that firms also report conservatively to defer tax costs.

Most studies of these explanations focus on conditional conservatism. This form of conservatism (e.g., impairment accounting) requires a higher degree of verification for good news than for bad news *after* difficult-to-verify news occurs (Basu 1997). Watts (2003) points out that conditional conservatism biases equity book value and earnings downward. In comparison, unconditional conservatism (e.g., excessive depreciation) also addresses difficult-to-verify information and biases the numbers downward; however, this form is applied *before* difficult-to-verify news occurs. Thus, it immunizes accounting systems against future bad news.[1]

This study explores the distinct, common, and interrelated roles of the two forms of conservatism.[2] This is important for two reasons. First, recent studies note that some proposed explanations do not apply to both forms. Basu (2005) suggests that taxation is likely to generate unconditional conservatism. Ball and Shivakumar (2005) argue that contracting generates conditional conservatism, which is partially supported by Ball et al.'s (2005) finding on debt contracting. Thus, while the two forms may play a common role (e.g., in litigation), they are also likely to play distinct roles (e.g., in contracting and taxation). Evidence of their distinct and common roles can help identify the desirable form(s) to meet a specific need.

Second, Beaver and Ryan (2005) demonstrate that unconditional conservatism (e.g., excessive depreciation) lowers book values, therefore reducing subsequent conditional conservatism (e.g., impairment write-downs). This role of the unconditional form in reducing the conditional form suggests that the two forms are negatively interrelated. Evidence of the interrelated role of the two forms, taken together with the evidence of their distinct roles, highlights a necessary trade-off between them.[3] For example, firms may sacrifice contracting efficiency (via conditional conservatism) for tax reduction (via unconditional conservatism). In addition, controlling for this negative interrelation can help to properly identify the effect of a given factor on conditional conservatism; otherwise, the effect is underestimated (overestimated) if the factor encourages (discourages) unconditional conservatism.

The four explanations of the two forms of conservatism and the interrelation of the two forms are unified as follows. Regulation and taxation provide incentives to record expenses and losses as early as possible, without requiring a particular form of conservatism to achieve this. Because unconditional conservatism has some advantages over conditional conservatism (e.g., recording losses earlier), these two factors tend to induce unconditional conservatism. As this form can fully preempt conditional conservatism—for instance, expensing R&D eliminates any future write-down—the two factors do not induce conditional conservatism. Like regulation and taxation, litigation sometimes simply provides incentives to record expenses and losses as early as possible, and thus induces unconditional conservatism. However, litigation sometimes induces conditional conservatism because some potential plaintiffs particularly demand this form for contracting purposes. Contracting only generates conditional conservatism because unconditional conservatism does not provide new information and adds noise to the payoffs to contracting parties. In sum, the hypotheses are as follows: (1) contracting induces conditional conservatism; (2) litigation induces both forms; (3) regulation induces unconditional conservatism; and (4) taxation induces the unconditional form.

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[1] I thank an anonymous referee for the insight that "[u]nconditional conservatism immunizes the accounting system against future bad news."

[2] Accounting conservatism as defined in this study includes conditional and unconditional conservatism but excludes recording no rents (e.g., historical cost accounting) and understating easy-to-verify numbers (e.g., delaying easy-to-verify gains or subtracting easy-to-verify income). See Section IV for a discussion of these exclusions.

[3] I thank the anonymous referee again for this insight.

Conditional (unconditional) conservatism is measured by downward biases in equity book value and earnings that result from conditionally (unconditionally) conservative accounting.[4] In univariate tests, unconditional conservatism is regressed on each hypothesized factor. In addition, conditional conservatism is regressed on each factor before and after controlling for unconditional conservatism. Consistent with unconditional conservatism preempting conditional conservatism, the coefficient on unconditional conservatism is always significantly negative. The coefficient on a factor sometimes changes substantially after the control, suggesting a need for the control. In multivariate tests, the preemption effect is controlled for and all factors are incorporated simultaneously.

The findings are as follows: (1) Firms bearing higher equity and debt contracting costs choose greater conditional conservatism. (2) Firms facing larger litigation costs choose greater conditional and unconditional conservatism (with the results for auditor litigation costs being weak). (3) Firms bearing higher accounting regulation costs choose greater unconditional conservatism. (4) Firms facing a larger reduction in tax costs choose greater unconditional conservatism. These findings indicate that the two forms of conservatism not only play a common role in avoiding litigation, but also play distinct roles. The conditional form improves contracting efficiency, while the unconditional form helps prevent regulation and defer taxes. Further, a significantly negative association between the two forms of conservatism suggests that they play an interrelated role. The combined evidence implies that because the two forms meet distinct needs but are negatively interrelated, it is necessary to trade them off.

This paper contributes to our understanding of conservatism in two ways. First, it provides new evidence of the distinct, common, and interrelated roles of the two forms of conservatism. This evidence helps identify (1) which form(s) should be used to meet a specific need, (2) whether a trade-off between the two forms is necessary, and (3) which factors should be considered in the trade-off. This evidence also allows researchers to properly assess the effect of a factor on conditional conservatism because unconditional conservatism is controlled for.

Second, by simultaneously investigating multiple conservatism factors rather than examining different factors in isolation, this paper disentangles effects of the correlated factors and, hence, provides evidence that enables researchers to accurately gauge the effect of each factor.[5] Conducting firm-level tests further helps to properly identify these effects. It has the advantage that it is easier to control for confounding factors at the firm level, alleviating omitted variable and endogeneity problems that are inherent in country-level and time-series tests (Sloan 2001; Holthausen 2003). Another advantage is that substantial variation in firm incentives and the considerable discretion available to firms allow for powerful tests at the firm level.[6]

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[4] See Section IV for a discussion of potential measurement error introduced by recording no rents and understating easy-to-verify numbers.

[5] Ball et al. (2003) note that properties of reported numbers are determined by firm incentives, which depend on many factors such as investor demand, regulation, and taxation. Bushman and Piotroski (2006) stress the importance of incorporating multiple factors in obtaining a complete understanding of the property in question. Holthausen (2003) suggests that examining multiple factors helps us properly gauge the effects of individual factors.

The remainder of the paper is organized as follows. Section II motivates the research question with prior research. Section III develops hypotheses. Section IV describes research design. Section V presents results. Section VI concludes.

II. PRIOR RESEARCH

The extant literature provides ample evidence of the various conservatism explanations. Researchers document greater conservatism among firms in common-law countries (Ball et al. 2000), public firms (Ball and Shivakumar 2005), firms facing more severe conflicts over dividend policy (Ahmed et al. 2002), and firms monitored by more independent boards (Beekes et al. 2004). All of this evidence is consistent with the contracting hypothesis. Several studies provide evidence of the litigation explanation. Huijgen and Lubberink (2005) find greater conservatism among U.K. firms that are cross-listed in the U.S. (a more litigious country), while Chandra et al. (2004) show greater conservatism, but not higher litigation risk, among high-tech firms. Greater conservatism is also observed during periods of higher auditor liability (Basu 1997), during quarters of audited reports (Basu et al. 2001b), and among firms hiring big auditing firms (Basu et al. 2001a). Consistent with the regulation explanation, Sivakumar and Waymire (2003) show that enforceable accounting rules induced conservatism among early 20th century railroads. Two recent studies examine multiple explanations at the country level. Ball et al. (2003) find that four East Asian countries with high-quality standards show low levels of conservatism, indicating that factors other than standards also affect conservatism. Bushman and Piotroski (2006) show that legal and political institutions at the economy level interact with contracting and litigation factors at the country level to influence conservatism.

While the existing evidence provides significant insights, it is limited in two ways. First, because most of the above studies focus on conditional conservatism, the evidence only allows researchers to assess the effect of a factor on this form of conservatism. But the factor may not affect unconditional conservatism in the same way. Thus, the evidence does not allow researchers to understand the distinct/common roles of the two forms of conservatism. Without such an understanding, it is difficult to determine which form(s) should be used to meet a specific need. Furthermore, if the two forms meet distinct needs but are negatively interrelated, then firms face a trade-off between them. Without an understanding of the distinct and interrelated roles, it is difficult to determine whether a trade-off is necessary and which factors should be considered in the trade-off. In addition, with the interrelation of the two forms omitted, even the effect of a factor on the conditional form cannot be properly identified.

Second, because most of the above studies focus on different factors in isolation, the evidence does not allow researchers to accurately assess the effect of each factor. Several factors likely exist or change jointly (e.g., co-existing contracting and taxation or co-changing litigation and regulation). Thus, it is unclear which factors cause the existence of conservatism in a given period, such as that before regulation and litigation regimes (Holthausen and Watts 2001), or cause an inter-temporal change in conservatism, such as its increase during periods of increased auditor liability (Basu 1997). Countries or firms likely differ in many aspects (e.g., governance, litigation, and regulation). Hence, it is unclear which factors cause a cross-sectional difference in conservatism, such as that between common-law and code-law countries (Ball et al. 2000) or that between U.S.-listed and non-U.S.-listed firms (Huijgen and Lubberink 2005).

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[6] For example, although firms within a country have the same regulatory system, firms of different sizes bear different levels of regulation risk and thus face different amounts of expected regulation costs. Similarly, firms of different levels of market volatility bear different levels of litigation risk and thus face different amounts of expected litigation costs. Hence, there is substantial variation in firm incentives for conservatism. In addition, firms have discretion in exercising conditional conservatism (e.g., selecting the timing and amount of write-downs) and applying unconditional conservatism (e.g., implementing rules and choosing methods, estimates, and assumptions), as elaborated in Givoly et al. (2006) and indicated by accounting choice and earnings management studies.

Overcoming these two limitations motivates this paper. To identify not only the effect of a factor on conditional conservatism, but also its effect on unconditional conservatism, this paper examines both forms of conservatism, after controlling for the preemption of conditional conservatism by unconditional conservatism. To accurately assess the effect of individual factors on conservatism, this paper investigates multiple factors simultaneously.

III. HYPOTHESES

Contracting

To mitigate agency problems (Jensen and Meckling 1976), equity and debt holders contract with firms based on accounting numbers. Firms thus have incentives to bias equity book value and earnings upward. This tendency poses potential losses to the investors. Watts (2003) argues that accounting conservatism counteracts this tendency and biases the numbers downward, thereby reducing the expected losses to investors who have asymmetric loss functions. Thus, conservatism improves contracting efficiency.

Ball and Shivakumar (2005) specify that conditional conservatism improves contracting efficiency. This form curbs negative net present value (NPV) projects, thereby improving the efficiency of equity contracting, and it quickly triggers debt covenant violations, thereby enhancing the efficiency of debt contracting. On the other hand, this form forgoes positive NPV projects (Guay and Verrecchia 2006) and retains unnecessary capital within a firm (Watts 2003), thereby reducing contracting efficiency.[7] Watts (2003) resolves this conflict by pointing out that, for equity (debt) holders with asymmetric loss functions, the benefits of avoiding negative NPV projects (preventing excessive dividends) outweigh the costs of forgoing positive NPV projects (retaining unnecessary capital), resulting in a net increase in contracting efficiency.

Unconditional conservatism, which lowers equity book value and earnings as well, can also curb negative NPV projects and quickly trigger debt covenant violations. However, Basu (2005) notes that this form does not utilize new information, and Ball and Shivakumar (2005) argue that this form adds noise to payoffs to contracting parties and thus could reduce contracting efficiency. Overall, contracting is expected to encourage the conditional form only. Investors can induce this form by imposing costs on firms via governance mechanisms.[8]

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[7] Guay and Verrecchia's (2006) argument that conditional conservatism forgoes positive NPV projects is the flip side of the argument that it curbs negative NPV projects. Under conservative accounting, a positive NPV project may cause near-term earnings to be negative, and future earnings may reflect the benefits of the project after the tenure of a manager. This can lower the manager's compensation, causing him to forgo the project. Similarly, Watts' (2003) argument that conditional conservatism retains unnecessary capital is the flip side of the argument that it quickly triggers debt covenants violation and, hence, prevents excessive dividends. When the return on equity provided by a firm is lower than the return that shareholders can earn elsewhere, paying dividends is of the best interests of shareholders. However, conservative accounting understates equity book value and earnings; the understated numbers, when used in debt covenants that constrain dividends, reduce firms' flexibility in paying dividends. Consequently, conservative firms may not be allowed to pay dividends when it is efficient to do so.

Litigation

Litigation also provides incentives for conservatism, as suggested by Kellogg's (1984) evidence that firms and auditors are far more likely to be sued for overstatements than for understatements. Firms thus bear costs associated with litigation against them. Further, auditors can pass on their litigation costs to firms by being less accommodating to aggressive accounting, by increasing auditing fees (Pratt and Stice 1994), by issuing unfavorable audit opinions, or by terminating relations with risky clients (Krishnan and Krishnan 1997). Hence, firms also bear litigation costs that auditors pass on to them.

Firms can minimize these expected litigation costs by recognizing bad news as early as possible, because early recognition of bad news can reduce the probability that class action lawsuits will be certified and can shorten the length of the class period when those lawsuits are certified. Unconditional conservatism is preferred to achieve the early recognition of bad news because it records losses earlier than conditional conservatism does. However, if some potential plaintiffs particularly demand conditional conservatism in contracting, litigation also induces this form. In sum, litigation is expected to generate both conditional and unconditional conservatism.

Regulation

To avoid blame from constituents, accounting standard-setters and regulators (hereafter, the regulators) have an incentive to respond to constituent demand. Accordingly, if their constituents demand conservatism, then the regulators tend to induce conservatism (Watts 2003). Furthermore, the form(s) of conservatism that the regulators induce would depend on (1) which form(s) their constituents demand, (2) whether the regulators understand the constituent demand, and (3) which form(s) the regulators prefer.

In general, constituents probably prefer the unconditional form because the conditional form generates big negative shocks (e.g., large write-downs), which are unlikely to be desirable. Similarly, the regulators probably prefer the unconditional form, because relative to smooth value decreases recognized before bad news occurs, large negative shocks recognized after bad news occurs are more likely to cause the regulators problems. However, some constituents tend to favor conditional conservatism for contracting purposes. Ball and Shivakumar (2005) present evidence of the historical ambivalence in accounting standards, and suggest that the ambivalence could reflect the regulators' confusion of the two forms of conservatism. Thus, although these constituents favor conditional conservatism, the regulators may not well understand this need and thus may not favor this form. Collectively, the regulators tend to induce unconditional conservatism. They can induce this form by imposing regulation costs on firms (e.g., costs due to restatements and SEC investigations).

Given that firms capture the regulators' intention to induce unconditional conservatism, they tend to adopt this form to prevent regulatory intervention. In addition, when explicit or implicit rate of return regulation is in use, the unconditional form is more likely to prevent regulatory intervention because this form results in fewer large negative shocks in income and, hence, a lower probability of rate violation. In contrast, it is not apparent that conditional conservatism can prevent regulatory intervention. Instead, this form results in large negative shocks in income and, thus, may even trigger regulatory intervention that would not otherwise occur. Overall, regulation is expected to induce unconditional conservatism only.

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[8] Specifically, equity holders can impose costs such as potential reduced compensation, slowed promotion, and shortened tenure via board monitoring. Similarly, debt holders can impose costs such as potential immediate repayment, additional covenants, increased collateral, and raised interest rates (Gopalakrishnan and Parkash 1995) via debt monitoring such as having a trustee, hiring bond-rating agencies, and setting tight covenants (Dichev and Skinner 2002).

Taxation

Shackelford and Shevlin (2001) summarize two book-tax links. First, tax-minimizing strategies often result in lower book income due to book-tax conformity. Second, because firms are concerned with large book-tax differences, they choose to lower book income when reducing taxable income. Because of these links, taxation tends to induce conservative accounting. Basu (2005) points out that firms' choices between the two forms of conservatism in tax planning result from cost-benefit trade-offs. He also presents historical evidence that taxation induces the unconditional form. I extend his point as follows.

From the perspective of the first link, the unconditional form is more likely to defer taxes. Tax codes often allow extra expenses due to unconditional conservatism (e.g., extra COGS due to LIFO) to be deductible; however, losses due to conditional conservatism are barely allowed to be deductible, perhaps because such losses are based on unrealized decreases in market value rather than realized losses from transactions. From the perspective of the second link, it is likely that firms prefer to use the unconditional form to reduce book income. Whereas both forms can lower book income, the unconditional form (1) recognizes losses earlier (i.e., before news occurs), (2) is easier to control (i.e., independent of news), (3) costs less to implement (e.g., no impairment test), and (4) results in smoother earnings (e.g., fewer shocks). Collectively, taxation is expected to induce the unconditional form only.

Hypothesis Tests

To test whether unconditional conservatism preempts conditional conservatism and whether this relationship affects hypothesis tests, I regress conditional conservatism on each factor before and after controlling for unconditional conservatism. Unconditional conservatism is also regressed on each factor. The cross-sectional regressions are as follows:

$$\begin{aligned} CSV_j^c &= \alpha_0 + \alpha_1 Cost_j + \varepsilon_j; \\ CSV_j^c &= \alpha_0 + \alpha_1 Cost_j + \alpha_2 CSV_j^u + \varepsilon_j; \text{ and} \\ CSV_j^u &= \beta_0 + \beta_1 Cost_j + \delta_j \qquad (1) \end{aligned}$$

where:

CSV_j^c = a conditional conservatism measure for firm j;
CSV_j^u = an unconditional conservatism measure for firm j;
$Cost_j$ = one of the following proxies for factors inducing conservatism for firm j:
  $CtrCost\_Eq_j$ = a proxy for equity contracting costs;
  $CtrCost\_Dt_j$ = a proxy for debt contracting costs;
  $LitiCost\_Fm_j$ = a proxy for firm litigation costs;
  $LitiCost\_Aud_j$ = a proxy for auditor litigation costs;
  $ReguCost_j$ = a proxy for regulation costs; and
  $TaxCost_j$ = a proxy for tax cost reduction.

The expected sign of α_1 is positive when *Cost* is *CtrCost_Eq*, *CtrCost_Dt*, *LitiCost_Fm*, or *LitiCost_Aud*. The expected sign of β_1 is positive when *Cost* is *LitiCost_Fm*, *LitiCost_Aud*, *ReguCost*, or *TaxCost*. The expected sign of α_2 is negative because unconditional conservatism is expected to preempt conditional conservatism. The following cross-sectional multivariate regressions are used to test the incremental effect of each factor:

$$\begin{aligned}
CSV_j^c = {} & \alpha_0 + \underset{(+)}{\alpha_1} CtrCost\_Eq_j + \underset{(+)}{\alpha_2} CtrCost\_Dt_j + \underset{(+)}{\alpha_3} LitiCost\_Fm_j \\
& + \underset{(+)}{\alpha_4} LitiCost\_Aud_j + \alpha_5 ReguCost_j + \alpha_6 TaxCost_j \\
& + \underset{(-)}{\alpha_7} CSV_j^u + \varepsilon_j; \text{ and} \\
CSV_j^u = {} & \beta_0 + \beta_1 CtrCost\_Eq_j + \beta_2 CtrCost\_Dt_j + \underset{(+)}{\beta_3} LitiCost\_Fm_j \\
& + \underset{(+)}{\beta_4} LitiCost\_Aud_j + \underset{(+)}{\beta_5} ReguCost_j + \underset{(+)}{\beta_6} TaxCost_j + \delta_j \qquad (2)
\end{aligned}$$

where all variables are as defined in Equation (1).

IV. RESEARCH DESIGN

Sample and Descriptive Statistics

The original sample consists of all Compustat 2004 industry and research firms in the period of 1982–2002, including an estimation period of 1982–87, a sample period of 1988–1999, and a sensitivity test period of 2000–02.[9] Among these firms, 884 firms meet the data requirements for book-value-based conservatism measures and 1,500 firms meet those for accruals-based conservatism measures.[10] Additional data requirements for all explanatory variables result in a final sample of 633 firms over the sample period of 1988–1999. Panel A of Table 1 presents descriptive statistics for key accounting and market variables. The statistics show that the final sample generally consists of well-established firms, indicating the presence of survivorship bias due to requirements for substantial data over a long period. To address survivorship bias, I use three larger samples over shorter sample periods: 719 firms over 1990–99, 825 firms over 1992–99, and 1,050 firms over 1994–99. In addition, I use two samples, 614 firms over 1988–2001 and 720 firms over 1990–2001, to address endogeneity.

Proxies for Factors Inducing Conservatism

Panel B of Table 1 reports descriptive statistics for proxies for factors inducing conservatism. Because an expected cost equals the probability of incurring a cost times the amount of the cost, expected costs imposed for contracting purposes increase with the probability that investors detect aggressive reporting. This probability is likely to be high when governance is strong, as suggested by the evidence that governance mechanisms constrain opportunistic reporting, including both the extreme tail of frauds (Dechow et al. 1996) and more subtle accruals management (Klein 2002). Therefore, stronger equity or debt governance proxies for higher expected costs imposed for equity or debt contracting

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[9] The sample period is constrained by my limited access to some databases, such as Compact D (used to estimate *CtrCost_Eq*) and SDC (see footnote 29).

[10] I exclude observations with a negative equity book value and those with the highest or lowest 1 percent value of any variable necessary to estimate a conservatism measure.

TABLE 1
Descriptive Statistics for the Final Sample of 633 Firms over the Sample Period of 1988–1999

Panel A: Descriptive Statistics for Key Accounting and Market Variables

	Balance Sheet Variables			Market Variables			Income Statement Variables						
Statistics	**Total Assets**	**PP&E**	**Book Value of Equity**	**Market Value of Equity**	**Book-to-Market**	**Annual Stock Return**	**Sales**	**Net Income**	**Operating Cash Flows**	**Total Accruals**	**Current Accruals**	**Depreciation**	**Net Unrealized Loss**
Mean	4492	0.94	1525	4754	0.60	0.04	1.79	0.04	0.11	−0.07	0.01	0.07	0.01
Std. Dev.	15732	0.79	4160	13396	0.29	0.10	1.50	0.04	0.07	0.07	0.03	0.05	0.02
t-value	7.18	29.85	9.23	8.93	51.45	9.63	30.07	26.22	41.84	−23.40	9.63	34.29	19.46
Q1	177	0.41	89	155	0.39	−0.01	0.86	0.03	0.07	−0.09	0.00	0.04	0.00
Median	605	0.71	305	652	0.56	0.05	1.38	0.05	0.10	−0.05	0.01	0.06	0.01
Q3	2451	1.20	1073	2696	0.76	0.10	2.31	0.07	0.14	−0.02	0.02	0.09	0.02

Total Assets = Compustat #6;
PP&E = gross property, plant, and equipment (#7)/market value of equity (#199 × #25);
Book Value of Equity = common equity (#60);
Market Value of Equity = closing price (#199) × common shares outstanding (#25);
Book-to-Market = Book Value of Equity/Market Value of Equity;
Annual Stock Return = (change in market value (#199 × #25) + dividend (#26 × #25) − net capital contribution (adjusted lagged price (#199/#27) × change in adjusted common shares outstanding (#25 × #27)))/Market Value of Equity;
Sales = net sales (#12)/Market Value of Equity;
Net Income = net income (#172)/Market Value of Equity;
Operating Cash Flows = net cash flows from operating activities (#308)/Market Value of Equity;
Total Accruals = Net Income − Operating Cash Flows;
Current Accruals = − (account receivable decrease (#302) + inventory decrease (#303) + account payable increase (#304) + tax payable increase (#305) + other asset and liability net change (#307))/Market Value of Equity;
Depreciation = depreciations and amortizations (#125)/Market Value of Equity; and
Net Unrealized Loss = − (special items (#17) + net realized loss (#213))/Market Value of Equity.

Firm-year variables are averaged over the sample period of 1988–1999 to obtain firm-specific variables.

(continued on next page)

TABLE 1 (continued)

Panel B: Descriptive Statistics for Proxies for Factors Encouraging Conservatism

Stat.	*CtrCost_Eq*	*CtrCost_Dt*	*LitiCost_Fm*	*LitiCost_Aud*	*ReguCost*	*TaxCost*
Mean	0.65	0.76	−0.18	0.95	0.62	0.77
Std. Dev.	0.15	0.27	0.67	0.20	0.49	0.31
t-value	109.16	71.66	−6.75	120.54	31.85	63.30
Q1	0.56	0.63	−0.69	1.00	0.00	0.58
Median	0.68	0.86	−0.16	1.00	1.00	0.85
Q3	0.76	0.99	0.27	1.00	1.00	0.99

CtrCost_Eq = the percentage of outsiders on the board calculated as the number of outsiders divided by that of directors; data are from the Compact D database; I identify directors listed in the "DO" (directors) section as outsiders based on data item "NAME" in the "NA" (officers) section, data item "TITLE" in the "DO" section, and data item "NAME" and "RELATION" in the "HN" (insiders) section;

CtrCost_Dt = the ratio of private long-term debt (Compustat #81 + #84 + #83) to total long-term debt (#82 + #81 + #84 + #83);

LitiCost_Fm = the first factor generated from a principal component analysis of five market variables: Equity Beta, Share Turnover, Market Value, Annual Return Skewness, and Return; the eigenvalue is 1.48 (30%); the eigenvector is (0.67, 0.66, 0.13, 0.02, 0.33); the final communality estimates are 66%, 64%, 3%, 0%, and 16%, respectively; Equity Beta is estimated from $r_{jd} - r_{fd} = \beta_{jt}(r_{md} - r_{fd}) + \varepsilon_{jd}$ $d = 1$ to days$_t$, where: r_{jd} is daily return of firm j in day d, r_{md} is daily return of equally weighted market index in day d, r_{fd} is daily risk-free rate, and days$_t$ is the number of trading days in year t; Share Turnover is $1 - (1 - Turn_{jt})^{days_t}$, where $Turn_{jt}$ is average daily trading volume divided by number of shares outstanding; Market Value is market value at fiscal year end; Return Skewness is the skewness of r_{jd} for each firm-year; Annual Return is $\Pi(1 + r_{jd}) - 1$ for each firm-year; all market data, except for daily risk-free rate, are from daily CRSP;

LitiCost_Aud = a binary variable that equals 1 if the code of a firm's auditor (Compustat #149) is from 1 to 8, and 0 otherwise;

ReguCost = a binary variable that equals 1 if sales deflated by industry total sales/the number of firms in the industry is of top quartile, and 0 otherwise; sales is Compustat #12; industry is based on two-digit SIC code; and

TaxCost = the association between book income and tax income, $\hat{\beta}_{1j}$, estimated from time-series regression $TX_{jt} = \beta_{0j} + \beta_{1j}BKTX_{jt} + \varepsilon_{jt}$ for firm j over the sample period, where $BKTX_{jt}$ is tax expense for firm j in year t (Compustat #16) and TX_{jt} is tax expense minus deferred tax expense (#50); all variables are deflated by lagged total assets (#6).

Firm-year variables are averaged over the sample period of 1988–1999 to obtain firm-specific variables.

purposes, respectively.[11] Following the corporate governance literature, I measure the power of equity governance by board independence.[12] The power of debt governance is measured as the proportion of private debt because, as Dichev and Skinner (2002) show, private debt holders have better governance mechanisms.

Expected costs imposed via litigation increase with the probability of being sued; higher litigation risk therefore proxies for larger litigation costs. Firm litigation risk is measured by the first principal component of five market variables (Equity Beta, Share Turnover, Market Value, Return Skewness, and Annual Return), as measured in Johnson et al. (2001). They suggest that this component, increasing with Equity Beta and Share Turnover, captures a firm's market volatility. Francis et al. (1994a) and Jones and Weingram (1996) find that firms with greater market volatility are more likely to be sued.[13] Auditor litigation risk is measured by an indicator variable for big auditing firms. Intuitively, these firms are perceived to have "deep pockets" and thus are more likely to be sued (Khurana and Raman 2004). In addition, litigation is more costly to these firms because they face larger damage awards and greater impairment to their reputation capital (Palmrose 1988). Consistent with big auditing firms bearing greater litigation costs, prior studies document that these firms are more likely to reduce the portion of risky clients during a period of increasing auditor liability (Jones and Raghunandan 1998) and to provide audit service of higher quality (Becker et al. 1998; Francis et al. 1999; Gul et al. 2006).

Expected costs imposed via accounting regulation increase with the probability that a firm is subject to accounting regulation; thus, higher regulation risk proxies for larger regulation costs. Regulation risk is measured by market share because firms with larger market share are more likely to be noticed and, hence, regulated. However, firms in an industry with fewer firms have larger market share by construction but may not be conspicuous; therefore, the number of firms in an industry is controlled for. In addition, a dummy variable, rather than a continuous variable, is used to reflect the idea that only firms with sufficiently large market share are conspicuous. Firm size variables such as total assets and market value are not used because (1) size likely proxies for many other firm characteristics and (2) by construction, size variables are correlated with conservatism measures used in this study.[14]

Because the expected reduction in tax costs increases with the strength of book-tax links, a stronger association between book income and taxable income proxies for a larger tax cost reduction. This association is estimated from time-series regressions of current income taxes (tax expense minus deferred tax expense) on tax expense. In summary, proxies for factors that induce conservatism are defined as follows:

CtrCost_Eq = the percentage of outsiders on the board (data from Compact D);
CtrCost_Dt = the ratio of private long-term debt (Compustat #81 + #84 + #83) to total long-term debt (#82 + #81 + #84 + #83);

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[11] Because governance mechanisms are chosen by investors and thus are endogenous, it is possible that greater conservatism leads to more efficient contracting and, hence, less reliance on governance. This may result in a negative equilibrium relationship between governance and conservatism. Although this endogeneity and the resulting negative relationship should be controlled for, it seems inappropriate to predict a negative underlying structural relationship that weak governance *causes* greater conservatism. It is greater conservatism that causes weaker governance, not the opposite.

[12] See Section V for a sensitivity analysis using alternative proxies for equity contracting costs.

[13] See Section V for a sensitivity analysis using alternative proxies for firm litigation costs.

[14] See Section V for a sensitivity analysis using alternative proxies for regulation costs.

LitiCost_Fm = the first factor generated from a principal component analysis of five market variables: Equity Beta, Share Turnover, Market Value, Return Skewness, and Annual Return (data from CRSP);
LitiCost_Aud = a binary variable that equals 1 if the code of a firm's auditor (Compustat #149) is from 1 to 8, and 0 otherwise;
ReguCost = a binary variable that equals 1 if sales deflated by industry total sales/the number of firms in the industry is of top quartile, and 0 otherwise; sales is Compustat #12; industry is based on two-digit SIC code; and
TaxCost = the association between book income and tax income estimated from time-series regressions of income taxes (Compustat #16 − #50) on tax expense (#16); all variables are deflated by lagged total assets (#6).

Firm-year variables are averaged over a sample period to obtain firm-specific variables.

Measures of Conservatism

Since both forms of conservatism bias equity book value and earnings downward, persistent understatements in these numbers are used to measure conservatism. However, two types of understatements that arise from tendencies beyond conservatism may introduce measurement error. First, Roychowdhury and Watts (2006) argue that if accounting is intended to record net separable asset value rather than equity value, then recording no rents (the difference between equity value and net asset value) cannot be considered as conservatism. However, recording no rents lowers book value, resulting in measurement error. In sensitivity tests, I control for the change in cash investments, a variable used by Easton and Pae (2004) to capture unrecorded rents. Second, understating easy-to-verify numbers also causes understatements. However, firms are unlikely to persistently understate easy-to-verify numbers, given that users of accounting numbers (including the IRS) can undo such understatements. Thus, there is only trivial measurement error even if this tendency is not regarded as conservatism.

Book-Value-Based Measures of Conservatism

Using market value as the benchmark, Beaver and Ryan (2000) measure conservatism as downward bias in book value. They estimate this measure using fixed firm and time effects regression of book-to-market on current and lagged returns. Given that a return proxies for contemporaneous news, the return terms capture lags in book value. The firm intercepts capture biases. As they restrain the coefficients on the returns to be symmetric between good and bad news, the return terms capture symmetric lags only; conditional bias due to asymmetric lags and unconditional bias are likely to be captured by the firm intercepts.

This study allows the coefficients on the returns to be asymmetric. Thus, the return terms also capture asymmetric lags; the firm intercepts only capture unconditional bias and are therefore used to measure unconditional conservatism. In fact, this book-value/returns approach is a cumulative version of the earnings/return approach used in Ball et al. (2005). Roychowdhury and Watts (2006) show that a cumulative approach is better.[15] Specifically, book-value-based measures of unconditional conservatism ($CSVBV^u$), conditional conservatism ($CSVBV^c$), and overall conservatism ($CSVBV$) are estimated using the following fixed firm and time effects regression on panel data over a sample period (T):[16]

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[15] They regress cumulative earnings on a cumulative return. Their cumulative approach is similar to the book-value/returns approach. However, they restrain coefficients on different annual returns to be the same, while these coefficients are unlikely to be the same, as discussed later. This paper allows the coefficients to be different.

$$CSVBV_j^u = 1 - \hat{\alpha}_j;$$

$$CSVBV_j^c = -\frac{1}{T}\sum_{t=1}^{T}\left(\sum_{\tau=0}^{6}\hat{\gamma}_\tau^0 DR_{jt-\tau}{*}RET_{jt-\tau} + \sum_{k=1}^{6}\sum_{\tau=0}^{6}\hat{\gamma}_\tau^k\, Cost_j^k{*}DR_{jt-\tau}{*}RET_{jt-\tau}\right);$$

$$CSVBV_j = CSVBV_j^u + CSVBV_j^c;\ \text{and}$$

$$\begin{aligned} BM_{jt} = {} & \alpha_j + \alpha_t + \sum_{\tau=0}^{6}\beta_\tau RET_{jt-\tau} + \sum_{\tau=0}^{6}\gamma_\tau^0 DR_{jt-\tau}{*}RET_{jt-\tau} \\ & + \sum_{k=1}^{6}\sum_{\tau=0}^{6}\gamma_\tau^k Cost_j^k{*}DR_{jt-\tau}{*}RET_{jt-\tau} + \mu_{jt} \end{aligned} \tag{3}$$

where:

$CSVBV_j^u$ = the book-value-based measure of unconditional conservatism for firm j;
$CSVBV_j^c$ = the book-value-based measure of conditional conservatism for firm j;
$CSVBV_j$ = the book-value-based measure of overall conservatism for firm j;
BM_{jt} = book-to-market for firm j at the end of fiscal year t = book value of equity (Compustat #60)/market value (#199 × #25);
RET_{jt} = annual return for firm j in fiscal year t = (change in market value (#199 × #25) + dividend (#26 × #25) − net capital contribution (adjusted lagged price (#199/#27) × change in adjusted common shares outstanding (#25 × #27)))/market value (#199 × #25);
DR_{jt} = a dummy variable that equals 1 if RET_{jt} is negative, and 0 otherwise; and
$Cost^k{}_j$ = the proxy for the kth conservatism factor for firm j, as defined in Equation (1).

To allow for asymmetric coefficients on returns, I interact each return (*RET*) with a dummy variable that indicates negative return (*DR*). The coefficient on a *DR**RET* reflects the degree of asymmetry in recording the *RET*. It is not the asymmetry in recording a single return, but the combined asymmetry in recording *all* returns, that fully reflects the level of conditional conservatism. Therefore, I measure conditional conservatism as a downward bias due to this combined asymmetry (i.e., as negative mean predicted book-to-market using *DR**RET* terms).

Further, to allow the degree of asymmetry to vary with conservatism factors, I interact each *DR**RET* with six *Cost* variables (i.e., proxies for the factors). Otherwise, (1) it would be the cross-sectional difference in returns, rather than that in *recording* returns, that drives the cross-sectional difference in the measure, and (2) a cross-sectional test of the determinants of conditional conservatism would become a test of the determinants of negative returns.

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[16] Because including both firm and time effects in the regression leads to perfect collinearity, I drop one of the time effects. Firm intercepts can thus be used to measure absolute rather than relative conservatism level.

Panel A of Table 2 presents statistics related to the estimation of the book-value-based measures. Intercepts and returns explain 76 percent of the variation in book-to-market (*BM*). The mean firm intercept is less than 1 (0.76), meaning that an average firm's understatement in book value (1 − BM) contains a bias component (1 − 0.76 = 0.24). Coefficients on RET_t, RET_{t-1}, RET_{t-2}, RET_{t-3}, RET_{t-4}, RET_{t-5}, and RET_{t-6} are −0.67, −0.45, −0.28, −0.19, −0.10, −0.07, and −0.05, respectively, meaning that the understatement also contains lags in recording positive returns (67 percent of RET_t, 45 percent of RET_{t-1}, 28 percent of RET_{t-2}, 19 percent of RET_{t-3}, 10 percent of RET_{t-4}, 7 percent of RET_{t-5}, and 5 percent of RET_{t-6} are unrecorded). The proportion of unrecorded positive return attenuates as the time lag increases, consistent with good news being gradually recorded over multiple years. The coefficient on DR_t*RET_t is 0.37, meaning that when RET_t is negative the proportion of unrecorded returns is only 30 percent (i.e., 0.67 − 0.37 = 0.30). This is consistent with the presence of conditional conservatism. As the time lag increases, the coefficient on a $DR*RET$ drops, suggesting that the degree of asymmetry diminishes as net unrecorded good news is subsequently recorded.

Column "Mean Coefficient Averaged over the Time Lags" in Table 2 reports statistics from tests of the null hypotheses that $\frac{1}{7}\sum_{\tau=0}^{6} \gamma_\tau^k = 0$ for each $k = 1$ to 6. These statistics show that coefficients on $Cost*DR*RET$ terms are on average significantly positive when *Cost* is *CtrCost_Eq*, *CtrCost_Dt*, or *LitiCost_Fm*, providing preliminary evidence that conditional conservatism increases with contracting and litigation costs. However, when *Cost* is *LitiCost_Aud*, the coefficients are on average insignificant, probably due to the lack of cross-sectional variation in *LitiCost_Aud* (95 percent of sample firms hire big auditing firms).

After the above measurement stage, a second stage is used to test the hypotheses, as in Equations (1) and (2). This two-stage approach has two advantages over a single-stage approach that combines measurement and hypothesis testing into one regression (e.g., Equation (3) with firm intercepts replaced by the *Cost* variables). First, $CSVBV^c$ combines the asymmetries in recording all returns; thus, when it is used as the dependent variable in the second stage, the coefficient on a *Cost* variable reflects the effect of a factor on the combined asymmetry. Conversely, under a single-stage approach, the coefficient on a $Cost*DR*RET$ only reflects the effect of a factor on the asymmetry in recording a single return. Second, the two-stage approach enables researchers to test explicitly the effect of unconditional conservatism on conditional conservatism, but researchers are unable to test this effect using a single-stage approach.

Despite these advantages, the two-stage approach is subject to a circularity concern. Specifically, the *Cost* variables affect the estimation of $CSVBV^c$ and then this estimate is regressed on the same *Cost* variables in order to test the hypotheses. The effect of a factor on conditional conservatism (an effect estimated in stage two) is essentially a transformation of the effects of the factor on the asymmetries in recording returns (effects estimated in stage one). To alleviate this concern, I also use a single-stage approach. Specifically, to test the effect of the factors on unconditional conservatism, I replace firm intercepts in Equation (3) with the *Cost* variables. Since a smaller firm intercept indicates greater unconditional conservatism, the coefficient on a *Cost* variable is expected to be significantly negative for factors that are expected to induce unconditional conservatism. To test the effect of the factors on conditional conservatism, I test whether the mean coefficient on $Cost^k*DR*RET$ terms (averaged across the time lags) is significantly different from zero for each $k = 1$ to 6.

TABLE 2
Book-Value-Based Measures of Conservatism Estimated Using a Fixed Firm and Time Effects Regression on Panel Data of 884 Firms over 1988–1999

Panel A: The Measurement Stage of the Two-Stage Approach

$$CSVBV_j^u = 1 - \hat{\alpha}_j;$$

$$CSVBV_j^c = -\frac{1}{T}\sum_{t=1}^{T}\left(\sum_{\tau=0}^{6} \hat{\gamma}_\tau^0 DR_{jt-\tau}{*}RET_{jt-\tau} + \sum_{k=1}^{6}\sum_{\tau=0}^{6} \hat{\gamma}_\tau^k Cost_j^{k}{*}DR_{jt-\tau}{*}RET_{jt-\tau}\right);$$

$$CSVBV_j = CSVBV_j^u + CSVBV_j^c; \text{ and}$$

$$BM_{jt} = \alpha_j + \alpha_t + \sum_{\tau=0}^{6} \beta_\tau RET_{jt-\tau} + \sum_{\tau=0}^{6} \gamma_\tau^0 DR_{jt-\tau}{*}RET_{jt-\tau} + \sum_{k=1}^{6}\sum_{\tau=0}^{6} \gamma_\tau^k Cost_j^{k}{*}DR_{jt-\tau}{*}RET_{jt-\tau} + \varepsilon_{jt}$$

Adjusted R^2 = 76%

	Firm	Year	RET_{t-6}	RET_{t-5}	RET_{t-4}	RET_{t-3}	RET_{t-2}	RET_{t-1}	RET_t
Mean	0.76***	0.03*	−0.05***	−0.07***	−0.10***	−0.19***	−0.28***	−0.45***	−0.67***
t	73.07	1.81	−4.08	−6.14	−7.62	−13.35	−19.39	−27.05	−36.85
n	884	12							
		Mean coefficient averaged over the time lags	$DR_{t-6}*RET_{t-6}$	$DR_{t-5}*RET_{t-5}$	$DR_{t-4}*RET_{t-4}$	$DR_{t-3}*RET_{t-3}$	$DR_{t-2}*RET_{t-2}$	$DR_{t-1}*RET_{t-1}$	DR_t*RET_t
			−0.05	−0.11**	−0.13***	0.10**	0.06	0.15***	0.37***
			−1.13	−2.16	−3.05	2.02	1.36	2.84	7.28
		$CtrCost\_Eq*DR*RET$	$CtrCost\_Eq*DR_{t-6}*RET_{t-6}$	$CtrCost\_Eq*DR_{t-5}*RET_{t-5}$	$CtrCost\_Eq*DR_{t-4}*RET_{t-4}$	$CtrCost\_Eq*DR_{t-3}*RET_{t-3}$	$CtrCost\_Eq*DR_{t-2}*RET_{t-2}$	$CtrCost\_Eq*DR_{t-1}*RET_{t-1}$	$CtrCost\_Eq*DR_t*RET_t$
		0.21***	0.28***	0.25***	0.29***	0.16***	0.19***	0.16***	0.11**
		12.14	5.29	4.22	5.72	2.83	3.45	2.99	2.17
		$CtrCost\_Dt*DR*RET$	$CtrCost\_Dt*DR_{t-6}*RET_{t-6}$	$CtrCost\_Dt*DR_{t-5}*RET_{t-5}$	$CtrCost\_Dt*DR_{t-4}*RET_{t-4}$	$CtrCost\_Dt*DR_{t-3}*RET_{t-3}$	$CtrCost\_Dt*DR_{t-2}*RET_{t-2}$	$CtrCost\_Dt*DR_{t-1}*RET_{t-1}$	$CtrCost\_Dt*DR_t*RET_t$
		0.06***	−0.02	0.09**	0.12***	0.05	0.08**	0.09***	0.00
		5.68	−0.70	2.51	3.60	1.36	2.37	2.75	0.08

(continued on next page)

TABLE 2 (continued)

*LitiCost_Fm** *DR*RET*	*LitiCost_Fm** DR_{t-6}*RET_{t-6}	*LitiCost_Fm** DR_{t-5}*RET_{t-5}	*LitiCost_Fm** DR_{t-4}*RET_{t-4}	*LitiCost_Fm** DR_{t-3}*RET_{t-3}	*LitiCost_Fm** DR_{t-2}*RET_{t-2}	*LitiCost_Fm** DR_{t-1}*RET_{t-1}	*LitiCost_Fm** DR_t*RET_t
0.03***	0.03***	0.03*	0.06***	0.02	0.00	0.06***	0.03*
7.51	2.70	1.66	3.63	1.37	0.21	4.65	1.86
*LitiCost_Aud** *DR*RET*	*LitiCost_Aud** DR_{t-6}*RET$_{t-6}$	*LitiCost_Aud** DR_{t-5}*RET$_{t-5}$	*LitiCost_Aud** DR_{t-4}*RET$_{t-4}$	*LitiCost_Aud** DR_{t-3}*RET$_{t-3}$	*LitiCost_Aud** DR_{t-2}*RET$_{t-2}$	*LitiCost_Aud** DR_{t-1}*RET$_{t-1}$	*LitiCost_Aud** DR_t*RET_t
0.01	−0.03	0.07**	0.00	0.04	0.07**	−0.04	−0.03
1.31	−0.97	2.37	0.09	1.32	2.08	−1.20	−0.77
*ReguCost** *DR*RET*	*ReguCost** DR_{t-6}*RET_{t-6}	*ReguCost** DR_{t-5}*RET_{t-5}	*ReguCost** DR_{t-4}*RET_{t-4}	*ReguCost** DR_{t-3}*RET_{t-3}	*ReguCost** DR_{t-2}*RET_{t-2}	*ReguCost** DR_{t-1}*RET_{t-1}	*ReguCost** DR_t*RET_t
−0.06***	−0.04*	−0.02	−0.12***	−0.03	−0.08***	−0.08***	−0.02
−9.05	−1.88	−0.91	−5.80	−1.54	−3.92	−4.39	−1.32
*TaxCost** *DR*RET*	*TaxCost** DR_{t-6}*RET_{t-6}	*TaxCost** DR_{t-5}*RET_{t-5}	*TaxCost** DR_{t-4}*RET_{t-4}	*TaxCost** DR_{t-3}*RET_{t-3}	*TaxCost** DR_{t-2}*RET_{t-2}	*TaxCost** DR_{t-1}*RET_{t-1}	*TaxCost** DR_t*RET_t
−0.14***	−0.14***	−0.21***	−0.10***	−0.21***	−0.12***	−0.09***	−0.12***
−19.74	−6.80	−8.41	−4.53	−7.85	−4.55	−3.53	−5.47

Panel B: The Measurement and Hypothesis Testing Stage of the Single-Stage Approach

$$BM_{jt} = \sum_{k=1}^{6} \alpha^k Cost_j^k + \alpha_t + \sum_{\tau=0}^{6} \beta_\tau RET_{jt-\tau} + \sum_{\tau=0}^{6} \gamma_\tau^0 DR_{jt-\tau} {*} RET_{jt-\tau} + \sum_{k=1}^{6} \sum_{\tau=0}^{6} \gamma_\tau^k Cost_j^k {*} DR_{jt-\tau} {*} RET_{jt-\tau} + \varepsilon_{jt}$$

Adjusted R² = 39%

	Year	RET_{t-6}	RET_{t-5}	RET_{t-4}	RET_{t-3}	RET_{t-2}	RET_{t-1}	RET_t
Mean	1.01***	−0.05***	−0.08***	−0.12***	−0.21***	−0.29***	−0.44***	−0.66***
t	59.67	−3.12	−4.91	−6.36	−10.72	−14.51	−19.55	−26.98
n	12							

(continued on next page)

TABLE 2 (continued)

	Mean coefficient averaged over the time lags	$DR_{t-6}*RET_{t-6}$	$DR_{t-5}*RET_{t-5}$	$DR_{t-4}*RET_{t-4}$	$DR_{t-3}*RET_{t-3}$	$DR_{t-2}*RET_{t-2}$	$DR_{t-1}*RET_{t-1}$	DR_t*RET_t
		−0.07 −0.95	−0.02 −0.30	−0.03 −0.46	0.27*** 3.85	0.11* 1.67	0.27*** 3.41	0.48*** 6.40
CtrCost_Eq	CtrCost_Eq* DR*RET	CtrCost_Eq* $DR_{t-6}*RET_{t-6}$	CtrCost_Eq* $DR_{t-5}*RET_{t-5}$	CtrCost_Eq* $DR_{t-4}*RET_{t-4}$	CtrCost_Eq* $DR_{t-3}*RET_{t-3}$	CtrCost_Eq* $DR_{t-2}*RET_{t-2}$	CtrCost_Eq* $DR_{t-1}*RET_{t-1}$	CtrCost_Eq* DR_t*RET_t
−0.10*** −3.31	0.17*** 8.44	0.32*** 4.18	0.18** 2.12	0.23*** 3.07	0.07 0.82	0.16** 2.00	0.13 1.63	0.10 1.29
CtrCost_Dt	CtrCost_Dt* DR*RET	CtrCost_Dt* $DR_{t-6}*RET_{t-6}$	CtrCost_Dt* $DR_{t-5}*RET_{t-5}$	CtrCost_Dt* $DR_{t-4}*RET_{t-4}$	CtrCost_Dt* $DR_{t-3}*RET_{t-3}$	CtrCost_Dt* $DR_{t-2}*RET_{t-2}$	CtrCost_Dt* $DR_{t-1}*RET_{t-1}$	CtrCost_Dt* DR_t*RET_t
0.08*** 5.35	0.08*** 6.28	0.04 0.93	0.20*** 3.92	0.17*** 3.53	0.01 0.21	0.10* 1.85	0.07 1.48	−0.06 −1.23
LitiCost_Fm	LitiCost_Fm* DR*RET	LitiCost_Fm* $DR_{t-6}*RET_{t-6}$	LitiCost_Fm* $DR_{t-5}*RET_{t-5}$	LitiCost_Fm* $DR_{t-4}*RET_{t-4}$	LitiCost_Fm* $DR_{t-3}*RET_{t-3}$	LitiCost_Fm* $DR_{t-2}*RET_{t-2}$	LitiCost_Fm* $DR_{t-1}*RET_{t-1}$	LitiCost_Fm* DR_t*RET_t
−0.07*** −10.06	0.06*** 10.93	0.05*** 2.85	0.07*** 2.96	0.09*** 4.10	0.04* 1.70	0.01 0.34	0.08*** 4.14	0.05** 2.44
LitiCost_Aud	LitiCost_Aud* DR*RET	LitiCost_Aud* $DR_{t-6}*RET_{t-6}$	LitiCost_Aud* $DR_{t-5}*RET_{t-5}$	LitiCost_Aud* $DR_{t-4}*RET_{t-4}$	LitiCost_Aud* $DR_{t-3}*RET_{t-3}$	LitiCost_Aud* $DR_{t-2}*RET_{t-2}$	LitiCost_Aud* $DR_{t-1}*RET_{t-1}$	LitiCost_Aud* DR_t*RET_t
−0.05** −2.52	−0.03*** −3.08	−0.12*** −2.98	−0.04 −0.83	−0.08* −1.89	0.02 0.53	0.10** 2.22	−0.09* −1.77	−0.04 −0.81
ReguCost	ReguCost* DR*RET	ReguCost* $DR_{t-6}*RET_{t-6}$	ReguCost* $DR_{t-5}*RET_{t-5}$	ReguCost* $DR_{t-4}*RET_{t-4}$	ReguCost* $DR_{t-3}*RET_{t-3}$	ReguCost* $DR_{t-2}*RET_{t-2}$	ReguCost* $DR_{t-1}*RET_{t-1}$	ReguCost* DR_t*RET_t
−0.17*** −16.89	−0.09*** −12.36	−0.09*** −3.14	−0.09*** −2.58	−0.17*** −5.57	−0.05 −1.47	−0.13*** −4.09	−0.09*** −3.43	−0.04 −1.58
TaxCost	TaxCost* DR*RET	TaxCost* $DR_{t-6}*RET_{t-6}$	TaxCost* $DR_{t-5}*RET_{t-5}$	TaxCost* $DR_{t-4}*RET_{t-4}$	TaxCost* $DR_{t-3}*RET_{t-3}$	TaxCost* $DR_{t-2}*RET_{t-2}$	TaxCost* $DR_{t-1}*RET_{t-1}$	TaxCost* DR_t*RET_t
−0.15*** −11.26	−0.19*** −22.37	−0.12*** −4.10	−0.24*** −6.73	−0.12*** −3.64	−0.29*** −7.41	−0.19*** −5.08	−0.17*** −4.71	−0.19*** −5.91

(*continued on next page*)

TABLE 2 (continued)

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*, **, *** Indicate that mean coefficient or a single coefficient is significantly different from zero at the 10 percent, 5 percent, and 1 percent levels, respectively.

$CSVBV_j^u$ = the book-value-based measure of unconditional conservatism for firm *j*;
$CSVBV_j^c$ = the book-value-based measure of conditional conservatism for firm *j*;
$CSVBV_j$ = the book-value-based measure of overall conservatism for firm *j*;
BM_{jt} = book-to-market for firm *j* at the end of fiscal year *t* = book value of equity (Compustat #60)/market value (#199 × #25);
RET_{jt} = annual return for firm *j* in fiscal year *t* (change in market value (#199 × #25) + dividend (#26 × #25) − net capital contribution (adjusted lagged price (#199/#27) × change in adjusted common shares outstanding (#25 × #27)))/market value (#199 × #25);
DR_{jt} = a dummy variable that equals 1 if RET_{jt} is negative, and 0 otherwise; and
$Cost_j^k$ = the proxy for the *k*th conservatism factor for firm *j*, as defined in Table 1.

Panel B of Table 2 presents results from the single-stage approach. Consistent with litigation, regulation, and taxation inducing unconditional conservatism, the coefficient on a *Cost* variable is significantly negative when *Cost* is *LitiCost_Fm*, *LitiCost_Aud*, *ReguCost*, or *TaxCost*: the coefficient (t-value) is −0.07 (−10.06), −0.05 (−2.52), −0.17 (−16.89), or −0.15 (−11.26), respectively. Results are also consistent with contracting and litigation inducing conditional conservatism, except for auditor litigation. Specifically, when *Cost* is *CtrCost_Eq*, *CtrCost_Dt*, or *LitiCost_Fm*, the coefficients on the *Cost***DR***RET* terms are on average significantly positive, with a mean coefficient (t-value) of 0.17 (8.44), 0.08 (6.28), or 0.06 (10.93), respectively. When *Cost* is *LitiCost_Aud*, the coefficients are on average significantly negative, with a mean coefficient (t-value) of −0.03 (−3.08), again, this is probably due to the lack of cross-sectional variation in the variable. Overall, these single-stage results provide support for the predicted relations.

Accruals-Based Measures of Conservatism

Givoly and Hayn (2000) measure conservatism as downward bias in cumulative earnings, using cumulative operating cash flows as the benchmark; in other words, they measure conservatism as negative cumulative accruals. It follows that negative cumulative accruals resulting from conditionally (unconditionally) conservative accounting can be used to measure conditional (unconditional) conservatism. Because unconditionally conservative accounting (e.g., overestimating bad debt, using the LIFO method, and overestimating depreciation) affects current and depreciation accruals, negative cumulative current and depreciation accruals are used to measure unconditional conservatism. Conditionally conservative accounting (e.g., impairment accounting for long-lived assets) influences unrealized gain and loss accruals, so cumulative net unrealized loss is used to measure conditional conservatism.

In two situations, impairment accounting for current assets may introduce measurement error to these accruals-based measures. First, if impairment write-downs of current assets appear on cash flow statements as changes in working capital accounts, then the unconditional conservatism measure captures conditional conservatism to some extent. However, the write-downs seem to be coded as another item in Compustat (#217), alleviating this problem. Second, if these write-downs appear on income statements as operating expenses rather than losses, then the conditional conservatism measure does not fully capture conditional conservatism. It is difficult to assess the extent of measurement error because it is unclear how firms report and how Compustat codes the items. Thus, I statistically examine these measures after estimating them and do not rely exclusively on them in hypothesis tests. Specifically, accruals-based measures of unconditional conservatism ($CSVAC^u$), conditional conservatism ($CSVAC^c$), and overall conservatism ($CSVAC$) are estimated using the following fixed industry effect (two-digit SIC) regressions on pooled data over a sample period (T):

$$\begin{aligned}
CSVAC_j^u &= \sum_{t=1}^{T} [-\hat{\varepsilon}_{jt} + \hat{\delta}_{jt}];\ CSVAC_j^c = \sum_{t=1}^{T} \hat{\nu}_{jt};\ CSVAC_j = CSVAC_j^u + CSVAC_j^c;\\
CA_{jt} &= \alpha_{1i} + \alpha_{2i}\Delta SALE_{jt} + \alpha_{3i}\Delta NOA_{jt} + \varepsilon_{jt};\\
DP_{jt} &= \beta_{1i} + \beta_{2i}PPE_{jt-1} + \beta_{3i}\Delta NOA_{jt} + \delta_{jt};\ \text{and}\\
UR_{jt} &= \gamma_{1i} + \nu_{jt} \qquad (4)
\end{aligned}$$

where:

$CSVAC_j^u$ = the accruals-based measure of unconditional conservatism for firm j;
$CSVAC_j^c$ = the accruals-based measure of conditional conservatism for firm j;
$CSVAC_j$ = the accruals-based measure of overall conservatism for firm j;
CA_{jt} = current accruals for firm j in fiscal year t = − (account receivable decrease (Compustat #302) + inventory decrease (#303) + account payable increase (#304) + tax payable increase (#305) + other asset and liability net change (#307));
$SALE_{jt}$ = net sales for firm j in fiscal year t (#12);
NOA_{jt} = net operating assets for firm j at the end of fiscal year t = common equity (#60) + debt in current liability (#34) + long-term debt (#9) + preferred stock (#130) − cash (#1) − other investments and advances (#32) + minority interests (#38);
DP_{jt} = depreciation and amortization for firm j in fiscal year t (#125);
PPE_{jt} = gross property, plant, and equipment for firm j at the end of fiscal year t (#7); and
UR_{jt} = net unrealized loss firm j in fiscal year t = − (special items (#17) + net realized loss (#213)).

All variables, including the intercepts, are deflated by market value (#199 × #25).

I make several adjustments to alleviate other types of measurement error. First, prior abnormal accruals studies find that current accruals increase with growth in sales (McNichols 2000); therefore, current accruals are adjusted for growth in sales. Second, because depreciation expense is included in earnings but its cash flow counterpart (e.g., cash for PP&E) is excluded from operating cash flows, depreciation accruals are adjusted for that cash flow counterpart. Third, Penman and Zhang (2002) find that accruals-based measures overstate (understate) the conservatism level of growth (declining) firms because of the accumulation (release) of hidden reserves. Current and depreciation accruals are therefore adjusted for growth in net operating assets. Finally, conservatism mandated by accounting standards affects accruals. Ahmed et al. (2002) argue that mandated conservatism is likely to affect firms in the same industry to the same extent. Each accruals component is thus adjusted for its industry mean.[17] Mandated conservatism in the form of expensing R&D introduces only trivial measurement error because both earnings and operating cash flows contain expensed R&D. Similarly, since rents are recorded in neither earnings nor operating cash flows, recording no rents introduces only trivial measurement error.

Table 3 reports statistics related to the estimation of the accruals-based measures. Like researchers using accruals models (McNichols 2000), I find that current accruals increase with current growth ($\Delta SALE$) and long-term growth (ΔNOA), and that depreciation increases with beginning PPE. The mean of industry-wide net unrealized loss (i.e., industry intercept) is significantly positive, consistent with the presence of mandated conditional conservatism. Overall, statistics in Tables 2 and 3 are reasonable, increasing our confidence in the measures.

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[17] Industry-wide *voluntary* conservatism is also likely to exist. For example, Francis et al. (1994b) find that some industries have higher litigation risk. This suggests that all firms in such an industry are likely to choose greater conservatism. Omitting industry-wide voluntary conservatism reduces the power of tests and biases against finding results.

TABLE 3
Accruals-Based Measures of Conservatism Estimated Using Fixed Industry Effect Regressions on Pooled Data of 63 Industries over 1988–1999

$$CSVAC_j^u = \sum_{t=1}^{T} [-\hat{\varepsilon}_{jt} + \hat{\delta}_{jt}];\ CSVAC_j^c = \sum_{t=1}^{T} \hat{v}_{jt};\ CSVAC_j = CSVAC_j^u + CSVAC_j^c;$$

$$CA_{jt} = \alpha_{1i} + \alpha_{2i}\Delta SALE_{jt} + \alpha_{3i}\Delta NOA_{jt} + \varepsilon_{jt};\ DP_{jt} = \beta_{1i} + \beta_{2i}PPE_{jt-1} + \beta_{3i}\Delta NOA_{jt} + \delta_{jt};\ \text{and}\ UR_{jt} = \gamma_{1i} + v_{jt}.$$

Dependent Variable	*CA* n = 75,627 firm-years Adjusted R^2 = 30%			*DP* n = 73,658 firm-years Adjusted R^2 = 58%			*UR* n = 76,651 firm-years Adjusted R^2 = 3%
Independent Variable	**Intercept**	**Δ*SALE***	**Δ*NOA***	**Intercept**	**lag *PPE***	**Δ*NOA***	**Intercept**
Mean	−0.100***	0.019***	0.161***	0.083***	0.075***	−0.001	0.250***
t-value	−2.97	3.35	11.24	4.63	25.85	−0.20	7.24
n (industry)	63	63	63	63	63	63	63

*** Indicates that mean coefficient is significantly different from zero at the 1 percent level.
Industry *i* is identified using two-digit SIC code.

$CSVAC_j^u$ = the accruals-based measure of unconditional conservatism for firm *j*;
$CSVAC_j^c$ = the accruals-based measure of conditional conservatism for firm *j*;
$CSVAC_j$ = the accruals-based measure of overall conservatism for firm *j*;
CA_{jt} = current accruals for firm *j* in fiscal year *t* = − (account receivable decrease (Compustat #302) + inventory decrease (#303) + account payable increase (#304) + tax payable increase (#305) + other asset and liability net change (#307));
$SALE_{jt}$ = net sales for firm *j* in fiscal year *t* (#12);
NOA_{jt} = net operating assets for firm *j* at the end of fiscal year *t* = common equity (#60) + debt in current liability (#34) + long-term debt (#9) + preferred stock (#130) − cash (#1) − other investments and advances (#32) + minority interests (#38);
DP_{jt} = depreciation and amortization for firm *j* in fiscal year *t* (#125);
PPE_{jt} = gross property, plant, and equipment for firm *j* at the end of fiscal year *t* (#7); and
UR_{jt} = net unrealized loss for firm *j* in fiscal year *t* = − (special items (#17) + net realized loss (#213));
All variables, including the intercepts, are deflated by market value (#199 × #25).

Comparisons among Measures of Conservatism

Panel A of Table 4 presents descriptive statistics for the conservatism measures. $CSVBV^c$ is significantly smaller than $CSVAC^c$ (an untabulated t-value of −9.86 from the paired t-test), with means (medians) of 0.07 (0.04) and 0.15 (0.08), respectively. This is possibly because $CSVBV^c$ understates the absolute value of a conditional bias. For a firm that adopts conditional conservatism, $CSVBV^c$ shows a bias only if a market return is negative (i.e., only if bad news dominates good news). However, a conditional bias does exist if good news equals bad news, and it may exist even if good news dominates bad news.[18]

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[18] For example, if a firm recognizes 33 percent of current good news and 70 percent of current bad news, and if \$100 good news and \$100 bad news occur in a year, then a conditional bias of \$37 (= 100 × 70% − 100 × 33%) is created in the year but $CSVBV^c$ shows no bias. Similarly, if \$100 good news and \$80 bad news occur in the year, then a conditional bias of \$23 (= 80 × 70% − 100 × 33%) is created but $CSVBV^c$ shows no bias.

TABLE 4
Comparison among Measures of Conservatism over 1988–1999

Panel A: Descriptive Statistics for Conservatism Measures Used in This Study

	Conditional Conservatism		Unconditional Conservatism		Overall Conservatism	
	Book-Value-Based Measure $CSVBV^c$	Accruals-Based Measure $CSVAC^c$	Book-Value-Based Measure $CSVBV^u$	Accruals-Based Measure $CSVAC^u$	Book-Value-Based Measure $CSVBV$	Accruals-Based Measure $CSVAC$
Mean	0.07	0.15	0.27	0.03	0.34	0.18
Std. Dev.	0.08	0.22	0.26	0.43	0.25	0.47
t-value	20.87	17.12	26.04	1.72	33.48	9.58
Q1	0.02	0.01	0.12	−0.14	0.22	−0.05
Median	0.04	0.08	0.30	0.05	0.37	0.15
Q3	0.09	0.23	0.46	0.23	0.51	0.40

Panel B: Correlations among Conservatism Measures Used in This Study

	Conditional Conservatism		Unconditional Conservatism		Overall Conservatism	
	$CSVBV^c$	$CSVAC^c$	$CSVBV^u$	$CSVAC^u$	$CSVBV$	$CSVAC$
$CSVBV^c$		0.371*** <.0001	−0.225*** <.0001			
$CSVAC^c$	0.371*** <.0001			−0.084** 0.0169		
$CSVBV^u$	−0.225*** <.0001			0.114*** 0.0021		
$CSVAC^u$		−0.084** 0.0169	0.114*** 0.0021			
$CSVBV$						0.069** 0.0417
$CSVAC$					0.069** 0.0417	

(continued on next page)

TABLE 4 (continued)

Panel C: Descriptive Statistics for Conservatism Measures Used in Prior Studies

	Conditional Conservatism		Unconditional Conservatism		Overall Conservatism	
	Earnings-Returns Association	**Negative Non-Operating Accruals**	**Hidden Reserves**	**Conservative Accounting Choices**	**Downward Bias in Book Value**	**Negative Total Accruals**
Mean	0.05	0.08	0.05	0.59	0.38	−0.12
Std. Dev.	0.38	0.22	0.09	0.63	0.27	0.31
t-value	3.12	8.72	11.52	16.14	35.92	−9.88
Q1	−0.09	−0.01	0.00	0.00	0.24	−0.27
Median	0.03	0.07	0.03	0.58	0.42	−0.10
Q3	0.17	0.17	0.07	1.00	0.57	0.05
n	575	625	390	289	633	625

Panel D: Correlations between Conservatism Measures Used in This Study and Those Used in Prior Studies

	Conditional Conservatism		Unconditional Conservatism		Overall Conservatism	
	Earnings-Returns Association	**Negative Non-Operating Accruals**	**Hidden Reserves**	**Conservative Accounting Choices**	**Downward Bias in Book Value**	**Negative Total Accruals**
$CSVBV^c$	0.086**	0.117***				
	0.0195	0.0018				
$CSVAC^c$	0.053	0.156***				
	0.1043	<.0001				
$CSVBV^u$			0.156***	0.240***		
			0.0010	<.0001		
$CSVAC^u$			0.142***	0.076*		
			0.0025	0.0977		
$CSVBV$					0.974***	0.008
					<.0001	0.4185
$CSVAC$					0.067**	0.260***
					0.0459	<.0001

(continued on next page)

TABLE 4 (continued)

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*, **, *** Indicate that a correlation is significantly greater or less than zero at the 10 percent, 5 percent, and 1 percent levels, respectively.

Panels B and D report Pearson correlations and one-tailed p-values (below each correlation).

Earnings-Returns Association = $\hat{\beta}_{1j}$ estimated from time-series regression $X_{jt}/P_{jt-1} = \alpha_{0j} + \alpha_{1j}DR_{jt} + \beta_{0j}R_{jt} + \beta_{1j}DR_{jt}R_{jt} + \delta_{jt}$ over the sample period for firm j, where X_{jt} is earnings per share (#53), R_{jt} is annual return calculated by $\Pi(1 + r_{jd} - r_{md}) - 1$, r_{jd} is daily returns of firm j in day d, r_{md} is daily returns of size-adjusted equally weighted market index in day d, and DR_{jt} is a dummy variable that equals 1 if $R_{jt} < 0$, and 0 otherwise;

Negative Non-Operating Accruals = the accumulation of (− Non-Operating Accruals) over the sample period, where Non-Operating Accruals = (net income (#172) − operating cash flows (#308) + (account receivable decrease (#302) + inventory decrease (#303) + account payable increase (#304) + tax payable increase (#305) + other asset and liability net change (#307)) + depreciations and amortizations (#125)) /beginning total assets (#6);

Hidden Reserves = the sum of R&D Reserve, Advertising Reserve, and Inventory Reserve,

where:

R&D Reserve = the accumulation of $RDRSV_{jt}$ over the sample period deflated by total assets, $RDRSV_{jt} = \sum_{\tau=0}^{t-1} RD_{j,t-\tau}\left(1 - \sum_{n=0}^{\tau} \delta_{i,n}\right)$, RD_{jt} is R&D expenses (#46), and $\delta_{i,n}$ is Lev and Sougiannis (1996) estimated amortization rate of R&D for industry i in the nth year after a R&D investment;

Advertising Reserve = the accumulation of $ADVRSV_{jt}$ over the sample period deflated by total assets, $ADVRSV_{jt} = \sum_{\tau=0}^{t-1} ADV_{j,t-\tau}\left(1 - \sum_{n=0}^{\tau} \lambda_n\right)$, ADV_{jt} is advertising expenses (#45), λ_n is *ad hoc* amortization rate of advertising in the nth year after an advertising investment, and $\lambda_1 = 2/3$ and $\lambda_2 = 1/3$ for all industries as in Penman and Zhang (2002);

Inventory Reserve = the accumulation of the change of LIFO reserves (#240) over the sample period deflated by total assets;

Conservative Accounting Choices = the sum of Accelerate Method for Depreciation, Deferral Method for Income Tax Credit, and LIFO Method for Inventory,

where:

Deferral Method for Tax Credit = a dummy variable that equals 1 if deferral method is used for investment tax credit (ANTFT15 = "TA"), and 0 otherwise;

Accelerated Method for Depreciation = a dummy variable that equals 1 if accelerate method is used for depreciation (ANTFT8 = "TC"), and 0 otherwise;

LIFO Method for Inventory = a dummy variable that equals 1 if LIFO method is used for inventory (#59 = 2), and 0 otherwise; for computer (SIC 8040, 8050–52) and electronics (SIC 8053, 8070–90) industries where FIFO is more conservative because of the deflation of inventory, $LIFO_{jt}$ equals 1 if FIFO method is used for inventory (#59 = 1), and 0 otherwise;

Downward Bias in Book Value = firm intercept estimated from fixed firm and time effects regression of book-to-market on current return and lagged returns of up to six years; and

Negative Total Accruals = the accumulation of (− Total Accruals) over the sample period, where Total Accruals = (net income (#172) − operating cash flows (#308))/beginning total assets (#6).

Other variables are as defined in Tables 2 and 3.

In contrast, $CSVBV^u$ is significantly larger than $CSVAC^u$ (an untabulated t-value of 12.48 from the paired t-test), with means (medians) of 0.27 (0.30) and 0.03 (0.05), respectively.[19] This is probably because $CSVAC^u$ underestimates and/or $CSVBV^u$ overestimates conservatism. On the one hand, $CSVAC^u$ could underestimate conservatism for two reasons: (1) $CSVAC^u$ only reflects the bias accumulated over the sample period rather than the entire period for which a firm has existed; and (2) $CSVAC^u$, which reflects a firm's conservatism level relative to the industry average, does not capture industry-wide voluntary conservatism (see footnote 17). Such underestimation reduces the power of a test when $CSVAC^u$ is used as the dependent variable. On the other hand, $CSVBV^u$ could overestimate conservatism because $CSVBV^u$ contains unrecorded rents and mandated conservatism.[20]

Although these conservatism measures are subject to various types of measurement error, correlations among the measures and their correlations with other conservatism measures increase our confidence. In Panel B of Table 4, the correlations between $CSVBV^c$ and $CSVAC^c$, between $CSVBV^u$ and $CSVAC^u$, and between *CSVBV* and *CSVAC* are significantly positive, suggesting consistency between the book-value- and accruals-based measures. The correlations between $CSVBV^c$ and $CSVBV^u$ and between $CSVAC^c$ and $CSVAC^u$ are significantly negative, consistent with unconditional conservatism preempting conditional conservatism.

Panel D of Table 4 presents correlations of each measure with other measures that may capture the same form(s) of conservatism. The conditional measures ($CSVBV^c$ and $CSVAC^c$) are positively correlated with Earnings-Returns Association (Basu 1997) and Negative Nonoperating Accruals (Givoly and Hayn 2000).[21] The unconditional measures ($CSVBV^u$ and $CSVAC^u$) are positively correlated with Hidden Reserves (Penman and Zhang 2002) and Conservative Accounting Choices (Zmijewski and Hagerman 1981). The overall measures (*CSVBV* and *CSVAC*) are positively correlated with Downward Bias in Book Value (Beaver and Ryan 2000) and Negative Total Accruals (Givoly and Hayn 2000).[22] Most correlations are statistically significant. However, some are of small magnitude, perhaps because some measures (e.g., Hidden Reserves and Conservative Accounting Choices) are not designed to measure the aggregate level of a form of conservatism, and because measures used in this study and those used in prior studies are subject to measurement error. Collectively, the correlations suggest that $CSVBV^c$ and $CSVAC^c$ ($CSVBV^u$ and $CSVAC^u$) are capable of capturing conditional (unconditional) conservatism.

V. RESULTS

Correlations

In Panel A of Table 5, correlations between the conservatism measures and explanatory variables generally support the predicted relations. The insignificant correlation between $CSVAC^u$ and *LitiCost_Aud*, again, is probably due to the lack of cross-sectional variation

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[19] Although $CSVAC^u$ and $CSVAC^c$ are based on residuals estimated from Equation (4), the means are not zero, because intercepts are deflated by market value and because the final sample is a subset of firms used in the regression.

[20] It is unlikely that $CSVAC^u$ contains unrecorded rents because rents are recorded in neither earnings nor operating cash flows. $CSVAC^u$ unlikely contains mandated conservatism because it is controlled for by industry intercepts.

[21] To obtain a firm-specific measure, I estimate the Earnings-Returns Association measure using time-series regressions rather than cross-sectional regressions. This may introduce noise to the measure and cause its low or insignificant correlation with the measures used in this study.

[22] Like Ahmed et al. (2002), I find an *insignificant* correlation between *CSVBV* and Negative Total Accruals. However, the correlation between *CSVBV* and *CSVAC* (a modified Negative Total Accruals measure) is significantly positive (0.069 with a one-tailed p-value of 0.0417). If *CSVBV* appropriately measures conservatism, then this significant correlation suggests that *CSVAC* is better than the original Negative Total Accruals measure.

TABLE 5
Correlations for the Final Sample of 633 Firms over 1988–1999

Panel A: Correlations between Measures of Conservatism and Proxies for Factors Encouraging Conservatism

	CtrCost_Eq (+)	*CtrCost_Dt* (+)	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost*	*TaxCost*
$CSVBV^c$	0.099***	0.130***	0.109***	0.080**	−0.298***	−0.407***
	0.0064	0.0005	0.0031	0.0227	<.0001	<.0001
$CSVAC^c$	0.121***	0.122***	0.105***	0.129***	0.048	−0.234***
	0.0012	0.0011	0.0040	0.0006	0.2321	<.0001

	CtrCost_Eq	*CtrCost_Dt*	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost* (+)	*TaxCost* (+)
$CSVBV^u$	0.152***	−0.043	0.325***	0.073**	0.353***	0.141***
	0.0001	0.2805	<.0001	0.0324	<.0001	0.0002
$CSVAC^u$	0.004	0.065	0.135***	0.046	0.139***	0.100***
	0.9228	0.1012	0.0003	0.1242	0.0003	0.0060

Panel B: Correlations among Proxies for Factors Encouraging Conservatism

	CtrCost_Eq	*CtrCost_Dt*	*LitiCost_Fm*	*LitiCost_Aud*	*ReguCost*	*TaxCost*
CtrCost_Eq		0.076*	0.200***	0.098**	0.252***	−0.077*
		0.0570	<.0001	0.0141	<.0001	0.0542
CtrCost_Dt	0.076*		0.100**	0.186***	0.149***	−0.086**
	0.0570		0.0115	<.0001	0.0002	0.0314
LitiCost_Fm	0.200***	0.100**		0.128***	0.404***	0.033
	<.0001	0.0115		0.0013	<.0001	0.4014
LitiCost_Aud	0.098**	0.186***	0.128***		0.148***	−0.113***
	0.0141	<.0001	0.0013		0.0002	0.0046
ReguCost	0.252***	0.149***	0.404***	0.148***		−0.060
	<.0001	0.0002	<.0001	0.0002		0.1295
TaxCost	−0.077*	−0.086**	0.033	−0.113***	−0.060	
	0.0542	0.0314	0.4014	0.0046	0.1295	

*, **, *** Indicate that a correlation is significantly different from zero at the 10 percent, 5 percent, and 1 percent levels, respectively.

Panel A reports Pearson correlations and p-values (below each correlation). It reports a one-tailed p-value if a correlation has a predicted sign and a two-tailed p-value otherwise.

Panel B reports Pearson correlations and two-tailed p-values (below each correlation).

All variables are as defined in Tables 1, 2, and 3.

in *LitiCost_Aud*. In Panel B, prevalent significant correlations among the explanatory variables suggest a need to examine multiple factors simultaneously.

Univariate Regression Results

Table 6 presents results from univariate regressions of Equation (1). Like correlations reported in Panel A of Table 5, associations reported in Table 6 generally support the predicted relations, except for the insignificant association between $CSVAC^u$ and *LitiCost_Aud*. Panel A presents results using CSV^c as the dependent variable before and after controlling for CSV^u. The coefficient on CSV^u is always significantly negative, consistent with

TABLE 6

Summary Statistics from Regressions of Conservatism Measures on Proxy for Individual Factor Encouraging Conservatism on the Final Sample of 633 Firms over 1988–1999

$CSV_j^c = \alpha_0 + \alpha_1 Cost_j + \varepsilon_j$; $CSV_j^c = \alpha_0 + \alpha_1 Cost_j + \alpha_2 CSV_j^u + \varepsilon_j$; and $CSV_j^u = \beta_0 + \beta_1 Cost_j + \delta_j$.

Panel A: Conditional Conservatism Regressions

Dependant Variable	Stat.	*CtrCost_Eq* (+)	*CtrCost_Dt* (+)	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost*	*TaxCost*
$CSVBV^c$	Adj. R^2	0.8%	1.5%	1.0%	0.5%	8.7%	16.4%
	Intercept	0.033**	0.038***	0.072***	0.038**	0.101***	0.155***
		2.27	3.85	20.97	2.31	19.68	18.83
	Cost	0.055***	0.041***	0.014***	0.034**	−0.051***	−0.111***
		2.50	3.30	2.75	2.01	−7.83	−11.19
	Adj. R^2	6.6%	6.2%	8.5%	5.7%	10.2%	19.2%
	Intercept	0.041***	0.060***	0.099***	0.051***	0.108***	0.165***
		2.87	5.73	19.79	3.18	19.73	19.72
	Cost	0.076***	0.038***	0.025***	0.041***	−0.043***	−0.104***
		3.50	3.13	5.05	2.50	−6.19	−10.60
	$CSVBV^u$ (−)	−0.079***	−0.071***	−0.094***	−0.075***	−0.044***	−0.055***
		−6.32	−5.70	−7.23	−5.99	−3.40	−4.73
$CSVAC^c$	Adj. R^2	1.3%	1.3%	1.0%	1.5%	0.1%	5.3%
	Intercept	0.034	0.073***	0.156***	0.013	0.136***	0.278***
		0.89	2.80	17.31	0.31	9.67	12.11
	Cost	0.176***	0.100***	0.034***	0.143***	0.021	−0.167***
		3.06	3.08	2.66	3.28	1.20	−6.04
	Adj. R^2	1.9%	2.0%	1.8%	2.2%	0.8%	5.6%
	Intercept	0.035	0.071***	0.158***	0.010	0.134***	0.276***
		0.92	2.72	17.55	0.24	9.53	11.99
	Cost	0.177***	0.104***	0.039***	0.148***	0.027	−0.162***
		3.07	3.24	2.99	3.39	1.51	−5.86
	$CSVAC^u$ (−)	−0.043**	−0.047***	−0.051***	−0.046**	−0.047**	−0.031*
		−2.15	−2.35	−2.53	−2.30	−2.32	−1.59

Panel B: Unconditional Conservatism Regressions

Dependant Variable	Stat.	*CtrCost_Eq*	*CtrCost_Dt*	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost* (+)	*TaxCost* (+)
$CSVBV^u$	Adj. R^2	2.1%	0.0%	10.4%	0.4%	12.3%	1.8%
	Intercept	0.098**	0.300***	0.290***	0.177***	0.152***	0.176***
		2.15	9.64	28.81	3.52	9.80	6.37
	Cost	0.261***	−0.041	0.125***	0.096**	0.187***	0.119***
		3.85	−1.08	8.63	1.85	9.46	3.58
$CSVAC^u$	Adj. R^2	−0.2%	0.3%	1.7%	0.1%	1.8%	0.8%
	Intercept	0.023	−0.051	0.046**	−0.066	−0.047*	−0.079*
		0.29	−0.98	2.57	−0.78	−1.69	−1.70
	Cost	0.011	0.106	0.088***	0.101	0.124***	0.141***
		0.10	1.64	3.43	1.16	3.53	2.52

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*, **, *** Indicate that a coefficient is significantly different from zero (no predicted sign) or significantly greater/less than zero (with predicted +/− sign) at the 10 percent, 5 percent, and 1 percent levels, respectively.
Table 6 reports coefficients and t-statistics (below a coefficient).
All variables are as defined in Tables 1, 2, and 3.

unconditional conservatism preempting conditional conservatism. Furthermore, controlling for CSV^u sometimes substantially increases the adjusted R^2 and alters the coefficient on a factor. For example, when $CSVBV^c$ is used as the dependent variable, the adjusted R^2 rises from 0.8 to 6.6 percent for *CtrCost_Eq* and from 1.0 to 8.5 percent for *LitiCost_Fm*. Coefficients on these variables change from 0.055 to 0.076 and from 0.014 to 0.025, respectively. However, when $CSVAC^c$ is used as the dependent variable, changes in the adjusted R^2 and coefficients are less apparent. This is probably because the control variable, $CSVAC^u$, may underestimate unconditional conservatism and capture conditional conservatism to some extent, as discussed in Section IV. Overall, results in Panel A of Table 6 suggest that unconditional conservatism preempts conditional conservatism and that omitting unconditional conservatism can result in biased coefficients.

Multivariate Regression Results

Table 7 presents results from multivariate regressions of Equation (2). Panel A reports main results on the final sample of 633 firms over 1988–1999. A comparison of the univariate and multivariate results shows that coefficients on many explanatory variables change substantially. For instance, when $CSVBV^c$ or $CSVBV^u$ is used as the dependent variable, the coefficient (t-value) on *LitiCost_Aud* drops from 0.041 (2.50) to 0.016 (1.19) or from 0.096 (1.85) to 0.049 (1.01), respectively. These changes confirm the need to examine multiple factors simultaneously. Consistent with unconditional conservatism preempting conditional conservatism, the coefficients on $CSVBV^u$ and $CSVAC^u$ remain significantly negative.

Consistent with equity contracting inducing conditional conservatism, coefficients on *CtrCost_Eq* are significantly positive for conditional conservatism measures; the coefficients (t-values) are 0.072 (3.96) for $CSVBV^c$ and 0.117 (2.02) for $CSVAC^c$. For unconditional measures, the coefficient on *CtrCost_Eq* is either marginally significant or insignificant, indicating that unconditional conservatism is unlikely to be desirable in equity contracting. Consistent with debt contracting inducing conditional conservatism, coefficients on *CtrCost_Dt* are significantly positive for $CSVBV^c$ and $CSVAC^c$, with coefficients (t-values) of 0.035 (3.43) and 0.069 (2.14), respectively. For unconditional measures, the coefficient on *CtrCost_Dt* is either insignificant or significantly negative, indicating that unconditional conservatism can even be undesirable in debt contracting.

The results are consistent with firm litigation inducing both conditional and unconditional conservatism. The coefficients on *LitiCost_Fm* are significantly positive, with coefficients (t-values) of 0.038 (8.67) for $CSVBV^c$, 0.034 (2.50) for $CSVAC^c$, 0.078 (5.12) for $CSVBV^u$, and 0.057 (2.02) for $CSVAC^u$. The results for auditor litigation are generally weak, with most coefficients on *LitiCost_Aud* being insignificant. Because 95 percent of the sample firms hire big auditing firms, cross-sectional variation in *LitiCost_Aud* is small. This may reduce the power of tests and weaken the results. As sample size increases, coefficients on *LitiCost_Aud* tend to become significant, as can be seen in Panel B of Table 7.

The prediction that regulation induces unconditional conservatism is supported by significantly positive coefficients on *ReguCost* for unconditional conservatism measures. The coefficients (t-values) are 0.146 (6.83) for $CSVBV^u$ and 0.098 (2.48) for $CSVAC^u$. For conditional conservatism measures, the coefficient on *ReguCost* is either significantly negative (−0.079 with a t-value of −12.54 for $CSVBV^c$) or insignificant. Two possible explanations are discussed next.

First, it is possible that firms bearing higher regulation costs tend to choose a *lower* level of conditional conservatism, because large negative shocks resulting from conditional conservatism may attract regulators' attention and trigger regulatory intervention that would

TABLE 7
Summary Statistics from Regressions of Conservatism Measures on Proxies for Multiple Factors Encouraging Conservatism

$$CSV_j^c = \alpha_0 + \alpha_1 CtrCost\_Eq_j + \alpha_2 CtrCost\_Dt_j + \alpha_3 LitiCost\_Fm_j + \alpha_4 LitiCost\_Aud_j + \alpha_5 ReguCost_j$$
$$+\alpha_6 TaxCost_j + \alpha_7 CSV_j^u + \varepsilon_j; \text{ and}$$
$$CSV_j^u = \beta_0 + \beta_1 CtrCost\_Eq_j + \beta_2 CtrCost\_Dt_j + \beta_3 LitiCost\_Fm_j + \beta_4 LitiCost\_Aud_j + \beta_5 ReguCost_j + \beta_6 TaxCost_j + \delta_j.$$

Panel A: Regressions on the Final Sample of 633 Firms over 1988–1999

Dependant Variable	Adj. R^2	Intercept	*CtrCost_Eq* (+)	*CtrCost_Dt* (+)	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost*	*TaxCost*	CSV^u (−)
$CSVBV^c$	38%	0.132***	0.072***	0.035***	0.038***	0.016	−0.079***	−0.110***	−0.040***
		6.60	3.96	3.43	8.67	1.19	−12.54	−12.58	−3.55
$CSVAC^c$	9%	0.072	0.117**	0.069**	0.034***	0.087**	−0.018	−0.149***	−0.041**
		1.13	2.02	2.14	2.50	2.00	−0.95	−5.40	−2.10

Dependant Variable	Adj. R^2	Intercept	*CtrCost_Eq*	*CtrCost_Dt*	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost* (+)	*TaxCost* (+)
$CSVBV^u$	19%	0.059	0.099	−0.099***	0.078***	0.049	0.146***	0.127***
		0.83	1.54	−2.80	5.12	1.01	6.83	4.16
$CSVAC^u$	3%	−0.176	−0.113	0.077	0.057**	0.056	0.098***	0.152***
		−1.36	−0.96	1.18	2.02	0.64	2.48	2.71

Panel B: Regressions on a Sample of 719 Firms over 1990–99

Dependant Variable	Adj. R^2	Intercept	*CtrCost_Eq* (+)	*CtrCost_Dt* (+)	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost*	*TaxCost*	CSV^u (−)
$CSVBV^c$	34%	0.170***	0.033**	0.032***	0.036***	0.022*	−0.071***	−0.140***	−0.025**
		8.18	1.68	3.13	8.22	1.53	−10.54	−14.59	−2.18
$CSVAC^c$	8%	0.067	0.088**	0.058**	0.029***	0.080**	−0.014	−0.130***	−0.044**
		1.32	1.83	2.30	2.70	2.32	−0.89	−5.61	−2.10

(continued on next page)

TABLE 7 (continued)

Dependant Variable	Adj. R²	Intercept	*CtrCost_Eq*	*CtrCost_Dt*	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost* (+)	*TaxCost* (+)
CSVBV^u	18%	0.049	0.190***	−0.083**	0.068***	0.001	0.152***	0.109***
		0.74	2.97	−2.49	4.89	0.03	7.24	3.56
CSVAC^u	5%	−0.225**	−0.077	0.047	0.061***	0.165***	0.067***	0.116***
		−2.49	−0.89	1.04	3.21	2.67	2.36	2.80

Panel C: Regressions Using Alternative Book Value Conservatism Measures

Dependant Variable	Adj. R²	Intercept	*CtrCost_Eq* (+)	*CtrCost_Dt* (+)	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost*	*TaxCost*	*CSV*^{u} (−)
CSVBVG^c	41%	0.117***	0.092***	0.035***	0.047***	0.011	−0.081***	−0.107***	−0.038***
		6.06	5.25	3.61	10.95	0.86	−13.28	−12.66	−3.33

Dependant Variable	Adj. R²	Intercept	*CtrCost_Eq*	*CtrCost_Dt*	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost* (+)	*TaxCost* (+)
CSVBVG^u	19%	0.109	0.020	−0.075**	0.073***	0.066*	0.147***	0.126***
		1.60	0.32	−2.19	4.92	1.41	7.09	4.26

Panel D: Regressions on a Sample of 614 Firms with Measures of Conservatism over 1990–2001 and Proxies for Factors Encouraging Conservatism over a Lagged Period of 1989–1999

Dependant Variable	Adj. R²	Intercept	*CtrCost_Eq* (+)	*CtrCost_Dt* (+)	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost*	*TaxCost*	*CSV*^{u} (−)
CSVBV^c	36%	0.151***	0.089***	0.069***	0.067***	0.038**	−0.090***	−0.136***	−0.113***
		5.00	3.06	4.45	10.02	1.94	−8.81	−9.52	−6.50
CSVAC^c	8%	0.029	0.185**	0.087**	0.072***	0.093**	−0.012	−0.152***	−0.066***
		0.34	2.31	2.04	3.89	1.74	−0.45	−3.87	−2.45

(*continued on next page*)

TABLE 7 (continued)

Dependant Variable	Adj. R^2	Intercept	*CtrCost_Eq*	*CtrCost_Dt*	*LitiCost_Fm* (+)	*LitiCost_Aud* (+)	*ReguCost* (+)	*TaxCost* (+)
$CSVBV^u$	19%	0.073	0.065	−0.149***	0.054***	0.030	0.178***	0.133***
		1.03	0.97	−4.15	3.50	0.67	7.86	4.01
$CSVAC^u$	4%	−0.310**	−0.024	0.068	0.068***	0.161**	0.083**	0.142***
		−2.47	−0.20	1.07	2.48	2.00	2.06	2.41

*, **, *** Indicate that a coefficient is significantly different from zero (no predicted sign) or significantly greater/less than zero (with predicted +/− sign) at the 10 percent, 5 percent, and 1 percent levels, respectively.
Table 7 reports coefficients and t-statistics (below a coefficient).
$CSVBVG^c$ ($CSVBVG^u$) is the alternative book-value-based measure of conditional (unconditional) conservatism estimated after controlling for $\Delta SALE$, ΔNOA, ΔCI, and ΔRD in Equation (3), as follows:

$$CSVBVG_j^u = 1 - \hat{\alpha}_j;\ CSVBVG_j^c = \frac{1}{12}\sum_{t=1}^{12}\left(\sum_{\tau=0}^{6}\hat{\gamma}_\tau^0 DR_{jt-\tau} * RET_{jt-\tau} + \sum_{k=1}^{6}\sum_{\tau=0}^{6}\hat{\gamma}_\tau^k Cost_j^{k*} DR_{jt-\tau} * RET_{jt-\tau}\right);\ \text{and}$$

$$BM_{jt} = \alpha_j + \alpha_t + \sum_{\tau=0}^{6}\beta_\tau RET_{jt-\tau} + \sum_{m=1}^{4}\sum_{\tau=0}^{6}\delta^m G_{jt-\tau}^m + \sum_{\tau=0}^{6}\gamma_\tau^0 DR_{jt-\tau} * RET_{jt-\tau} + \sum_{m=1}^{4}\sum_{\tau=0}^{6}\lambda^{0m} DR_{jt-\tau} * G_{jt-\tau}^m + \sum_{k=1}^{6}\sum_{\tau=0}^{6}\gamma_\tau^k Cost_j^{k*} DR_{jt-\tau} * RET_{jt-\tau}$$

$$+ \sum_{m=1}^{4}\sum_{k=1}^{6}\sum_{\tau=0}^{6}\lambda^{km} Cost_j^{k*} DR_{jt-\tau} * G_{jt-\tau}^m + \varepsilon_{jt}$$

where:

G^1 = $\Delta SALE$ = the change in net sales (#12)/market value (#199 × #25);
G^2 = ΔNOA = the change in net operating assets (common equity (#60) + debt in current liability (#34) + long-term debt (#9) + preferred stock (#130) − cash (#1) − other investments and advances (#32) + minority interests (#38))/market value (#199 × #25);
G^3 = ΔCI = the change in cash investments (capital expenditures (#128) − proceeds from sale of property, plant and equipment (#107) + acquisitions (#129) − other investing activities (#310))/market value (#199 × #25); and
G^4 = ΔRD = the change in research and development expense (#46)/market value (#199 × #25).
Variables measured at the firm-year level are averaged over the sample period to obtain measures at the firm level.
Other variables are as defined in Tables 1, 2, and 3.

not otherwise occur. This negative association is less apparent for $CSVAC^c$, probably because this measure sometimes excludes the conditional bias due to write-downs of current assets.

Second, measurement error may drive the results. If unconditional conservatism is not fully captured by its measures, then its negative effect on conditional conservatism is not fully controlled for and the remaining effect causes the coefficient on *ReguCost* to be negative. Since the coefficient is more negative when $CSVBV^u$ is controlled for, this interpretation implies that $CSVBV^u$ would capture a lower level of unconditional conservatism. However, as discussed in Section IV, it is $CSVAC^u$ that tends to capture a lower level of unconditional conservatism. Thus, it is unlikely that error in unconditional conservatism measures drives the results. Another possibility is that error in conditional conservatism measures (especially error in $CSVBV^c$) is negatively correlated with *ReguCost*. Error in $CSVBV^c$ that is introduced by unrecorded rents, mandated conservatism, and growth is unlikely to drive the results, because results hold after controlling for these factors. Error introduced by the aggregation of news (see footnote 18) can drive the results, if conspicuous firms are more likely to have offsetting news.

The expectation that tax costs induce unconditional conservatism is supported by significantly positive coefficients on *TaxCost* for unconditional conservatism measures. The coefficients (t-values) are 0.127 (4.16) for $CSVBV^u$ and 0.152 (2.71) for $CSVAC^u$. For conditional conservatism measures, coefficients (t-values) on *TaxCost* are significantly negative: −0.110 (−12.58) for $CSVBV^c$ and −0.149 (−5.40) for $CSVAC^c$. Two possible explanations are (1) that conditional conservatism is uncontrollable, introduces volatility, and is therefore not neutral but undesirable in tax planning, or (2) that measurement error in $CSVBV^c$ that is introduced by offsetting news drives the results, as discussed above. The latter seems unlikely because using $CSVAC^c$ generates similar results.

Collectively, the findings are as follows: (1) contracting induces conditional conservatism; (2) litigation induces conditional and unconditional conservatism (with the results for auditor litigation being weak); (3) regulation induces unconditional conservatism; and (4) taxation induces unconditional conservatism. These findings indicate that the two forms of conservatism play a common role in litigation and play distinct roles in contracting, regulation, and taxation. Furthermore, the significantly negative association between the two forms suggests an interrelated role of them. Taken together, these findings imply that because the two forms are used for different purposes but are negatively interrelated, it is necessary to trade them off.

Sensitivity Tests

Sample Selection Bias

Firms in the final sample are those that survive through the 12-year sample period and the six-year lagged period. To address survivorship bias, I repeat tests on larger samples over shorter periods. Panel B of Table 7 provides multivariate results on a sample of 719 firms over 1990–99. The results are similar to the main results in Panel A. Inferences remain unchanged on samples of 825 firms over 1992–99 and 1,050 firms over 1994–99. Although these results alleviate the concern with survivorship bias, firms in these larger samples are still likely to be survivors.

Measurement Error

Several factors introduce measurement error into $CSVBV^c$ and $CSVBV^u$. First, hidden reserves are overstated (understated) for growth (declining) firms (Penman and Zhang

2002). To alleviate this growth-dependent bias, I control for change in sales and change in net operating assets. Second, unrecorded rents do not result from conservatism, but they reduce book value. To capture unrecorded rents, Easton and Pae (2004) include change in cash investments in a return regression. Accordingly, I control for change in cash investments in order to exclude unrecorded rents.[23] Third, mandated conservatism (e.g., expensing R&D) is not subject to firm choices, but it reduces book value. To exclude mandated conservatism, I control for change in R&D. Panel C of Table 7 reports results from regressions of Equation (2) using the conservatism measures estimated after controlling for the above variables in Equation (3). Alternatively, I control for these variables directly in Equation (2). In addition, like biased book value, biased market value—for example, due to market mispricing—can introduce measurement error. Thus, I control in Equation (3) for lead returns of up to three years and use the conservatism measures thus obtained in regressions of Equation (2). Overall, these analyses do not alter inferences, alleviating the concern of measurement error.

Alternative Proxy

Since *CtrCost_Eq* (board independence) captures only one aspect of equity governance, it is replaced by (1) an index of *CtrCost_Eq*, outsider ownership, and institutional ownership or (2) an index of those same three variables plus block holder ownership and board size.[24] I also identify gray directors as outsiders and recalculate the above variables.[25] *LitiCost_Fm* (the first principal component of five market variables) is replaced by each major individual variable (Equity Beta or Share Turnover). Using the above alternative proxies does not alter inferences.

I also use several variations of *ReguCost*—a variable indicating top quartile market share after controlling for industry size. First, I use market share without controlling for industry size and obtain insignificant results sometimes, probably because firms having large market share in a small industry are not conspicuous. Second, using a continuous market share variable sometimes yields insignificant results, consistent with the idea that only firms with sufficiently large market share are conspicuous. Last, using size variables often yields insignificant results, perhaps because firm size captures many other firm characteristics and/or because its relationship by construction to the conservatism measures complicates the tests. Nevertheless, including a size variable in regressions does not alter the inferences regarding other factors, alleviating the concern that the observed effects of other factors are manifestations of the effect of size.

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[23] Feltham and Ohlson (1996) develop the dynamics of cash flows and predict that price is a function of operating assets, abnormal earnings, lagged operating assets, and cash investments. They prove that the coefficient on cash investments increases with the NPV of cash flows generated from current cash investments. In addition, if investments to be made in future periods increase with the current investments (i.e., a positive autocorrelation of investments), then this coefficient also increases with the NPV of cash flows generated from the future investments. Easton and Pae (2004) use the change version of this price model. In particular, they regress stock return on earnings, change in earnings, dividends, lagged change in operating assets, and change in cash investments. The coefficient on change in cash investments in the return model is the same as the coefficient on cash investments in the price model. Following these two studies, I add to Equation (3) change in cash investments to capture the NPV that is incorporated in stock returns but not in the book (i.e., unrecorded rents).

[24] Data are from the Compact D database. Institutional ownership is data item "%OWN" for type="INSTITUTIONS" in the "SH" (ownership) section. Outsider ownership is the sum of the shareholdings of outsiders (based on data item "SHARES HELD" in the "HS" section) divided by the number of shares outstanding listed in the "CS" section. Block holder ownership is data item "%OWN" for type="5% OWNERS" in the "SH" (ownership) section. Board size is the number of directors on the board, as listed in the "DO" (directors) section.

[25] I gratefully acknowledge the set of hand-collected gray director data from Carol Liu.

Opportunistic Benefit

While firms tend to report conservatively in order to lower costs, they tend to report aggressively for contracting- and market-based benefits.[26] The supplied conservatism level results from the trade-off between costs and benefits. Thus, it is necessary to control for the benefits. Because managers with a stronger compensation/earnings association and firms with a tighter restriction on retained earnings are more likely to benefit from an overstatement,[27] compensation/earnings association and restrictiveness on retained earnings are used to proxy for contracting-based benefits.[28] Because firms with more equity and debt issuances are more likely to benefit from an overstatement, net equity issuances and debt issuances are used to proxy for market-based benefits.[29] Inferences remain unchanged after these controls.

Endogeneity

Because unconditional conservatism is endogenous, the equations in Equation (2) should be estimated using a simultaneous equation approach. However, estimating them as an equation system is infeasible because the system is not of full rank. Thus, I replace the first equation with six equations, each regressing conditional conservatism on a *Cost* variable after controlling for unconditional conservatism. These conditional conservatism equations, as well as the second equation in Equation (2), are estimated using the two-stage least squares method. For $CSVBV^c$, coefficients (t-values) on *CtrCost_Eq*, *CtrCost_Dt*, *LitiCost_Fm*, and *LitiCost_Aud* are 0.122 (4.68), 0.032 (2.50), 0.083 (6.95), and 0.055 (3.00), respectively; for $CSVAC^c$, the coefficients (t-values) are 0.177 (3.07), 0.120 (3.39), 0.073 (3.37), and 0.159 (3.47), respectively. These significantly positive coefficients confirm the main results that contracting and litigation induce conditional conservatism. In addition, the coefficients on $CSVBV^u$ and $CSVAC^u$ are again significantly negative.

Another endogeneity problem is that dependent variables in Equation (2) are likely to affect the independent variables. For example, the level of conservatism supplied by firms can influence the degree of monitoring selected by shareholders (*CtrCost_Eq*) and that chosen by debt holders (*CtrCost_Dt*). Similarly, the supplied conservatism level may affect litigation costs imposed by investors (*LitiCost_Fm*) and auditor choice made by managers (*LitiCost_Aud*). Ignoring this endogeneity will result in biased coefficients (even coefficients of the wrong sign) because they represent equilibrium relationships instead of underlying structural relationships. To address this problem, I measure conservatism over

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[26] Contracting-based incentives include attempts to increase bonuses (Healy 1985), stock-based compensation (Beneish 1999; Safdar 2003), and job security (DeFond and Park 1997), and attempts to avoid violating debt covenants *ex ante* (Dichev and Skinner 2002) and/or *ex post* (Sweeney 1994; DeFond and Jiambalvo 1994). Market-based incentives include attempts to reduce the cost of capital at equity issuances (Teoh et al. 1998a, 1998b; Rangan 1998), to avoid reporting losses or earnings decreases (Burgstahler and Dichev 1997), and to meet analyst forecasts (Degeorge et al. 1999).

[27] To illustrate, for a firm with a tighter restriction on retained earnings, an overstatement is more likely to enable the firm to pay excessive dividends and to avoid covenant violations. I do not use financial leverage because it is affected by financing policies and because Dichev and Skinner (2002), among others, show that it is a noisy proxy for debt-contracting-based benefits.

[28] Compensation/earnings association is estimated from time-series regressions of total compensation of top officers on net income (Compustat #172). Total compensation is data item "Remuneration" in section "NA" (officers) in the Compact D database. All variables are deflated by lagged total assets (#6). Restrictiveness on retained earnings is the ratio of restricted retained earnings (#36 − #38 − #97) to total common equity (#60).

[29] Net equity issuances are common stock issuances minus stock repurchases; data are from the SDC database. Common stock issuances are issuances of deal types "C" (public common), "PC" (private common), "R144C" (144A common), and "SHC" (shelf common). Stock repurchases are deals of transaction form "B" (buyback). Debt issuances include issuances of deal types "D" (straight debt), "CD" (convertible debt), "R144D" (144A straight debt), "R144CD" (144A convertible debt), "PD" (straight private debt), "PCD" (convertible private debt), "M" (mortgage), "TM" (municipal), and "SHD" (shelf debt).

1990–2001 and measure the cost variables over a *lagged* period of 1988–1999, resulting in a sample of 614 firms. I use these measures in regressions of Equation (2) and obtain similar results (see Panel D of Table 7). Results change little when I use another sample of 720 firms with conservatism measured over 1992–2001 and the cost variables measured over 1990–99. In fact, the endogeneity problem can be better addressed by estimating Equation (2) within a simultaneous equation system, given well-specified models for all endogenous variables. Since a fully exposited simultaneous equation analysis requires substantial space, I conduct this analysis in a separate study.

VI. CONCLUSIONS

This paper examines whether each proposed explanation for accounting conservatism applies to conditional conservatism, unconditional conservatism, or both. This is tested by (1) the regression of conditional conservatism on six factors after controlling for unconditional conservatism and (2) the regression of unconditional conservatism on the factors. Results lead to the following conclusions. First, contracting induces conditional conservatism, suggesting that this form of conservatism improves contracting efficiency. Second, firm litigation induces both conditional and unconditional conservatism, suggesting that both forms help avoid investor litigation against firms. Third, regulation induces unconditional conservatism, indicating that this form helps prevent regulatory intervention. Finally, taxation induces unconditional conservatism, indicating that this form is preferable in deferring taxes. Collectively, these two forms of conservatism not only play a common role in litigation, but also play distinct roles in contracting, regulation, and taxation. Furthermore, the two forms are negatively interrelated. Since they meet distinct needs but are negatively interrelated, it is necessary to trade them off.

This study has several limitations. First, since the samples likely consist of well-established firms, results may not generalize to less-established firms. Second, some measures used in this study may contain nontrivial errors. The book-value-based measure of conditional conservatism is subject to error introduced by offsetting news. The accruals-based conservatism measures underestimate conservatism because of limited accumulation period and excluded industry-wide voluntary conservatism; in addition, some conditional biases may be included in the unconditional measure but excluded from the conditional measure. The measure of debt contracting costs using Compustat data is likely to be noisy. Third, this paper proposes but does not discriminate among possible interpretations of the negative association of conditional conservatism with regulation and tax costs. Future studies can investigate these interpretations. Finally, this study addresses endogeneity using a parsimonious approach, but it can be better addressed using a simultaneous equation analysis.

Despite these limitations, this paper provides extensive firm-level evidence that allows us to properly gauge the importance of each conservatism explanation. More importantly, this paper not only reinforces the findings of prior studies that focus solely on conditional conservatism, but also provides new evidence that helps to identify the distinct, common, and interrelated roles of the two forms of conservatism.

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Editorial Data

The following table contains information about turnaround time for manuscripts (including revisions) on which editorial decisions were made in the 12-month period ended December 31, 2006. Turnaround time is the number of days between the date that the manuscript was received ***and the date of the editor's letter to the author(s).***

	Number of Manuscripts	Percent	Cumulative Number	Cumulative Percent
0 ≤ Days ≤ 30	68	16.23	68	16.23
31 ≤ Days ≤ 60	173	41.29	241	50.96
61 ≤ Days ≤ 90	110	26.25	351	83.77
91 ≤ Days ≤ 120	52	12.41	403	96.18
121 ≤ Days	16	3.82	419	100.00
Total	419	100.00		

The mean review time was 58.66; the median review time was 56 days.

New Submissions by calendar year (January 1 to December 31)

1992	241
1993	234
1994	231
1995	195
1996	230
1997	195
1998	196
1999	239
2000	260
2001	260*
2002	324
2003	327
2004	307
2005	389
2006	413

* 2001—in addition to the 260 regular submissions, 68 MSs were submitted to the *TAR* Quality of Earnings Conference.

The acceptance rate (defined as number of MSs accepted divided by the number of new submissions) over the past 5 years is between 12–16% per year.

EDITORIAL POLICY AND STYLE INFORMATION

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According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal ... No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and associate editors are responsible for providing critically constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation. Associate editors also make editorial recommendations to the editor.

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The Accounting Review's manuscript preparation guidelines follow *The Chicago Manual of Style* (15th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's Collegiate Dictionary*.

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1. All manuscripts should be printed in 12-point font on one side of 8½ × 11" good quality paper and be double-spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins of at least one inch from top, bottom, and sides should facilitate editing and duplication.
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Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: *three days; 3 kilometers; 30 years*. All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: *a well-presented analysis; re-form*. See *Webster's* for correct usage.

Keywords: The abstract is to be followed by four keywords that will assist in indexing the paper.

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An Abstract of about 100 words should be presented on a separate page immediately preceding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings. Keywords and the Data Availability statements should follow the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the Introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

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Fabozzi, F., and I. Pollack, eds. 1987. *The Handbook of Fixed Income Securities*. 2nd edition. Homewood, IL: Dow Jones-Irwin.
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305 LOYOLA MARYMOUNT UNIVERSITY, Department of Accounting invites applicants for two full-time tenure-track faculty positions at Assistant/Associate rank effective Fall 2007. A doctorate (or short-term ABD status) in accounting from an AACSB-accredited university is required. All candidates should demonstrate a strong commitment to scholarly research and excellence in teaching. Our priority is for teaching in the Financial Accounting and AIS areas. Salary is competitive and research support is available. If you are interested in learning more about these positions, contact: Professor Mahmoud Nourayi, Department of Accounting, College of Business Administration, Loyola Marymount University, One LMU Drive, Los Angeles, CA 90045-2659. Loyola Marymount, a comprehensive university in the mainstream of American Catholic higher education, seeks professionally outstanding applicants who value its mission and share its commitment to academic excellence, the education of the whole person, and the building of a just society. LMU is an Equal Opportunity institution actively working to promote an intercultural learning community. Women and minorities are encouraged to apply. For more information, go to: http://www.lmu.edu.

306 UNIVERSITY OF MIAMI, Department of Accounting invites applications for a Full Professor position for Fall 2007. Candidates must have the doctoral degree and a demonstrated record of teaching and research excellence. Preference will be given to candidates with teaching interests in financial accounting and auditing. The salary is competitive, and support for travel and research is available. Review of applications will begin immediately and continue until the position is filled. Interested applicants should submit via regular mail or email an application letter, *vita*, and three references (names, addresses, phone numbers, email) to: Dr. Oscar J. Holzmann, Chair, Accounting Faculty Recruiting Committee, School of Business Administration, 5250 University Drive KE 301, Coral Gables, FL 33146-6531 (holzmann@miami.edu). The University of Miami is an Equal Opportunity/Affirmative Action Employer.

307 MISSISSIPPI STATE UNIVERSITY COLLEGE OF BUSINESS AND INDUSTRY, School of Accountancy, invites applicants to apply for an Endowed Chair (Full Professor) or Endowed Professorship (Associate or Full Professor) tenure-track position with a specialization in financial accounting to begin Fall 2007 (ideally) or Fall 2008. Candidates must have an accounting doctorate degree, demonstrated excellence in teaching, publication record commensurate to that of an Endowed Chair or Professorship, and experience in working with doctoral students. A primary responsibility is to assist with leadership/involvement in the School's doctoral program. Professional certification and accounting experience are highly desirable. Compensation is highly competitive, according to qualifications and experience. Mississippi State University, located in Starkville, MS, offers a unique university town setting with a great environment to live and work. Send letter of interest, current *curriculum vitae*, and names (including contact information) of three references to: Chair, Search Committee, School of Accountancy, Drawer EF, Mississippi State, MS 39762, Position will remain open until filled. MSU is an AA/EEO Employer.

308 MASSACHUSETTS INSTITUTE OF TECHNOLOGY, Sloan School's Accounting Group seeks new faculty at all levels (Assistant, Associate, and Full Professor) for the academic year beginning July 2007, or as soon as possible thereafter. Applicants will be evaluated on their potential for and/or record of excellence in research and teaching. Please include evidence of research such as a publication, working paper, or dissertation proposal with other application materials. The deadline for applications is March 1, 2007. However, applications will be evaluated as soon as they are complete, if submitted at an earlier date. Applications must include a cover letter, *curriculum vitae*, and letters of recommendation. Applications should be sent to: Professor S. P. Kothari, Chair, Accounting Group Search Committee, MIT Sloan School of Management, 50 Memorial Drive, E52-325, Cambridge, MA 02142-1347. MIT is an Equal Opportunity Employer committed to building a culturally diverse intellectual community and strongly encourages applications from women and minorities.

309 MONTANA STATE UNIVERSITY–BOZEMAN, College of Business seeks to hire one academically qualified faculty member in the Accounting area, to start August 2007. All ranks considered. Applicants should be able to teach in at least one of the following areas: auditing, cost accounting, financial accounting, governmental accounting, or information systems. Candidate must have or anticipate receiving a Ph.D./D.B.A. with an emphasis in Accounting from an AACSB-accredited university. Candidate must exhibit the potential for teaching excellence, a capacity to produce quality research, and a commitment to involvement in service to the university and the profession. Interested individuals should submit a letter of interest, *curriculum vitae*, a statement of research interests, and a statement of teaching philosophy, teaching evaluations, and contact information for three professional/academic references to: Dr. Priscilla Wisner, College of Business, PO Box 173040, Montana State University, Bozeman, MT 59717-3040. Review of applications begins February 20, 2007 and continues until the position is filled. For more information, contact Dr. Wisner at either: Email: pwisner@montana.edu; Phone: (406) 994-2773; Website: http://www.montana.edu/msuinfo/jobs/faculty/. ADA/AA/EO/Veteran's Preference.

310 BOSTON COLLEGE, Wallace E. Carroll School of Management Department of Accounting invites applications for a faculty position at the Associate/Full Professor rank for Fall 2007. The qualified candidate must have a record of and continuing interest in conducting significant scholarly research and teaching excellence. We seek individuals with an openness to diverse intellectual interests and an appreciation for Boston College's liberal arts tradition. We offer a collegial environment and an energetic faculty with close ties to the Boston financial and academic communities. Direct inquiries and applications to: Billy Soo, Chair, Department of Accounting, Carroll School of Management, Boston College, Chestnut Hill, MA 02467. The Carroll School of Management is an Equal Opportunity Employer.

311 GEORGE MASON UNIVERSITY, School of Management, invites applications and nominations for Accounting faculty tenure-line positions at all ranks. Special consideration will be given to candidates with teaching and research interests in Accounting Information Systems. Successful candidates will have a Ph.D. and a strong commitment to excellence in teaching and scholarly research. Located in Fairfax, Virginia just outside Washington, D.C., the School of Management and Accounting Program are accredited by AACSB-International. The School of Management offers undergraduate degrees in business, including accounting, and the M.S.A., M.B.A., E.M.B.A., and Technology Management degrees at the graduate level. Applicants must apply online for position F7097Z at: http://jobs.gmu.edu. Letters of nomination or recommendation should be mailed or submitted electronically to: Dr. Sarah E. Nutter, Accounting Area Chair, George Mason University, MSN-5F4, 4400 University Dr., Fairfax, VA 22030; Email: acct-appt@gmu.edu. Information about the School of Management is at: http://www.som.gmu.edu.

312 THE UNIVERSITY OF AKRON continues its search for a tenure-track tax faculty position beginning August 2007. Rank and salary are negotiable. Expectations include teaching graduate-level courses, research and publishing, and playing a leadership role in the School's Master of Taxation program. A Ph.D. is the preferred academic credential. Individuals with L.L.M. (Taxation) are encouraged to apply. Preference will be given to candidates with professional certification, significant taxation experience, and ability to work closely with the professional community. Applicants must demonstrate a commitment to teaching excellence and conducting publishable tax research. The School of Accountancy offers B.S., M.S., and M.T. programs. Send a cover letter, names of three references, and *vita* to: Thomas G. Calderon, Chair, George W. Daverio School of Accountancy, College of Business Administration, The University of Akron (http://www.uakron.edu), Akron OH 44325-4802; Phone: (330) 972-6228; Email: tcalderon@uakron.edu. Review of applications will be ongoing until the position is filled.

313 THE NORWEGIAN SCHOOL OF ECONOMICS AND BUSINESS ADMINISTRATION (NHH) is seeking one or more faculty members for a full-time, tenure-track position in the Department of Accounting, Auditing, and Law. The position is initially for four years. We will consider applicants with interests in all areas of accounting research. Applicants should have, or be close to completing, a Ph.D. in accounting, economics, finance or a related field, and should have a strong research record or potential. The teaching load is one course per semester, which may be taught in English or Norwegian, and may be at the Bachelor, Master's, or Ph.D. level. Interested individuals should send *curriculum vitae*, research papers and three letters of recommendation to: Professor Frøystein Gjesdal, Department of Accounting, Auditing and Law, NHH, Helleveien 30, N-5045 Bergen, Norway; Email: froystein.gjesdal@nhh.no. We prefer applications to be sent electronically. For information on NHH, visit our website: http://www.nhh.no/index-e.html.

314 AVAILABLE FALL 2007. CPA (California and Hawaii), Ph.D. (Education, Accounting Cognate; University of Hawaii), M.S.B.A. (Finance; University of Southern California), M.B.A. (University of California, Irvine), B.A./B.S. (Mathematics/Accounting; University of Southern California). AACSB professionally qualified. Significant teaching experience in multiple subjects, especially in Financial Accounting and Managerial Accounting, as well as Math and Finance. Excellent evaluations. Ten-plus years or part-time and 15-plus years of full-time faculty experience from community college to graduate school. Ten-plus years of professional experience. Dissertation was qualitative in accounting education. Current research interests are quantitative in managerial accounting. Preferences: Southern California, Hawaii or Southwest, but will consider all locations. Prefer tenure-track, but will consider all full-time faculty positions. Phone: (949) 675-2047; Email: Jinkens@hawaii.edu.

315 EAST CAROLINA UNIVERSITY invites applications for a tenure-track Assistant/Associate Professor position in Accounting beginning in the 2007–2008 academic year. Applicants should be (or expect to be) terminally qualified (Ph.D. in Accounting or J.D.) by August 2007, and appropriate professional certification. Our current needs include a variety of specialties, but an emphasis is placed on tax and financial accounting. A 12 s.h. load is standard with release time granted for research and publication productivity. Send cover letter that describes specific skills and experience relevant to this position, current resume/*vita*, evidence of research/publication activity, current teaching evaluations, and three original letters of reference to: Dr. Dan Schisler, Chair, East Carolina University, 3208-A Bate Building, Greenville, NC 27858-4353. Official transcripts are required upon employment. Applicants must comply with the Immigration Reform and Control Act. ECU is an Equal Opportunity/Affirmative Action University and accommodates individuals with disabilities.

316 POSITION WANTED. I am looking for a part-time teaching, research position. I am retiring this academic year. Since 2003 I have published 12 publications (four empirical) and made six presentations. I am definitely AQ! If interested, contact me at: gmiller5@csub.edu or 661-654-6553. Let's talk!!

317 UNIVERSITY OF NEVADA, LAS VEGAS, College of Business, an AACSB-accredited college at both undergraduate and graduate levels, invites applications for an Assistant Professor in Accounting to start Fall 2007. The position requires a primary interest in accounting information systems and involves teaching at the undergraduate and graduate levels, conducting significant research leading to high-quality refereed publications, and performing service activities. Candidates must have a strong academic record and have demonstrated potential for excellence in both research and teaching. A doctoral degree in accounting from an AACSB-accredited school is required, but ABDs near completion will be considered. Salary competitive; contingent on market. This position is contingent upon funding. Application details may be obtained by visiting: http://hr.unlv.edu/Employment_Svcs. UNLV is an Equal Opportunity/Affirmative Action Employer committed to excellence through diversity.

318 SOUTHERN CALIFORNIA EDISON invites applications for a Tax Manager (Ref.# JP26884-JN); Location: Rosemead, CA. Requirements: Bachelor Degree in Business, Accounting, Finance or similar field. Alternatively, a Master's Degree in Taxation, a law degree or CPA certificate. 10+ years of corporate tax experience in an accounting firm, law firm, or large corporation. Proven experience in dealing with the IRS at the field audit level or in appeals. M.B.A./M.A./M.S. degree preferred. The selected individual will be responsible for managing all of the IRS income tax audits of EIX. This will include being the principal spokesman at weekly meetings, insuring that Information Document Requests are handled in an accurate and timely manner, and that necessary coordination occurs with the Mission Companies and various SCE departments. We offer competitive pay and excellent benefits. If you are interested in this position and to learn more, please submit your resume in confidence at: http://www.edisonjobs.com. EOE.

319 WICHITA STATE UNIVERSITY invites applications for a tenure-eligible faculty position beginning August 2007. Assistant, Associate, or Visiting Professor status considered. Required: Ph.D. /D.B.A. in Accounting (ABD considered); academically qualified for AACSB; teaching and research interests in accounting; commitment to quality teaching and research; commitment to professional community interaction; and commitment to diversity. Desirable: professional accounting experience; professional certification; teaching and research interests in accounting (auditing and/or accounting information systems especially desirable), consistent with the needs of the School of Accountancy. Applications should be submitted by April 2, 2007, though the position will remain open until filled. The position is contingent upon funding. Apply online at: jeffrey.bryant@wichita.edu, or send a *curriculum vitae* to: Jeff Bryant, School of Accountancy, Wichita State University, 1845 Fairmount, Wichita, KS 67260-0087; Phone: 316-978-6261. Wichita State University is an Equal Opportunity/Affirmative Action Employer.

320 SOUTHERN CONNECTICUT STATE UNIVERSITY, Accounting Department, invites applications for one tenure-track faculty position. Appointment would be at the rank of Associate or Assistant Professor, depending upon qualifications and experience. Preference will be given to candidates who can teach tax at both graduate and undergraduate levels, with additional teaching responsibilities in introductory courses. Additional responsibilities include productive service and scholarly activities. Qualifications: Ph.D. or D.B.A. in Accounting or ABD nearing completion from an AACSB-accredited school. L.L.M. with an accounting degree will also be considered. Screening begins December 4, 2006 and continues until the position is filled. Candidates should send letters of application, current *curriculum vitae*, and contact information of three references to: Chair, Search Committee, Department of Accounting, Southern Connecticut State University, 501 Crescent Street, New Haven, CT 06515. SCSU is an Affirmative Action/Equal Opportunity Employer and strongly encourages women and minority group members to apply.

321 MARIST COLLEGE seeks an Assistant/Associate Professor of Accounting beginning August 2008. Candidates must have a commitment to excellence in teaching and substantive research in accounting. Candidates with either an interest or experience in teaching financial accounting are preferred. This tenure-track position will require teaching at both the undergraduate and graduate levels (including online courses) as well as maintaining a high level of professional activity through research and service. Candidates should have an earned doctorate in accounting or a related field, or an anticipated completion date during the Fall 2008. Professional experience and certification is a definite plus. The business and accounting programs in the School of Management are fully accredited by AACSB International. Review of applications will begin immediately. Please submit a current *vitae*, letter of interest, and contact information for least three professional references via email to: human.resources@marist.edu. Indicate position in subject line.

322 UNIVERSITY OF RICHMOND invites applications for the position of Visiting Professor of Accounting for the 2007–08 academic year. The Accounting Department will also accept applications for one or more tenure-track faculty positions in Accounting beginning the Fall 2008. Candidates with appropriate experience and scholarship may be considered for appointment at a senior rank. Although all areas will be considered, preferred teaching areas include Financial Accounting, Auditing and Accounting Information Systems. A Ph.D. and evidence/promise of both strong teaching effectiveness and scholarship are required. Review of applicants will begin immediately and continue until the position is filled. Qualified candidates should send a letter of application and current *curriculum vitae*, including the names and addresses of three references, to: Dr. Raymond Slaughter, Chairman, Dept. of Accounting, Robins School of Business, University of Richmond, One Gateway Road, Richmond, VA 23173. For more information, visit our website at: http://www.richmond.edu; Email: rslaught@richmond.edu. The University of Richmond is an AA/EOE and specifically invites applications from women and minorities.

323 DREXEL UNIVERSITY, LeBow College of Business, the Department of Accounting is seeking candidates for a tenure-track faculty position at the Assistant Professor rank. All teaching and research areas will be considered with starting dates beginning September 1, 2007. Entry-level positions require the expectation of completing the Ph.D. before the beginning of the contract period. LeBow College of Business is fully accredited and offers undergraduate accounting, M.S. Accounting, M.B.A. with an accounting concentration, and a Ph.D. with an accounting emphasis. Recognized by *Financial Times*, *U.S. News & World Report*, and *Entrepreneur* magazines for superior programs, LeBow College is in a period of exponential growth in quality, achievement, and reputation. The College has a rich tradition of interfacing with the Philadelphia and Northeast/ Mid-Atlantic business communities through our well-established undergraduate co-op program. Applicants should submit *vitas*, working papers, teaching evaluations, and other supporting materials to: Professor Barbara Grein, Department of Accounting and Tax, LeBow College of Business, Drexel University, 3141 Chestnut Street, Philadelphia, PA 19104. Electronic submissions are encouraged: bgrein@drexel.edu.

324 FLORIDA INTERNATIONAL UNIVERSITY is seeking visiting, clinical, or tenure-track tax faculty for a position beginning Fall 2007. Rank and salary are negotiable. Preference will be given to candidates with a Ph.D. Individuals with an L.L.M. (Taxation) are also encouraged to apply. Expectations include teaching graduate-level courses, research, and publishing. The School of Accounting is separately accredited by the AACSB and offers a B.Acc., a M.Acc., and an M.S.T. There are approximately 1,000 accounting students at the baccalaureate level and 400 graduate students. FIU is a public university that admitted its first class in 1972. It is a Carnegie Doctoral Research-Extensive (Research-I) institution with enrollments in excess of 38,000 students. Send a *vita* and names of five references to: Sharon Lassar, Florida International University, School of Accounting, 11200 SW 8th St., RB 239, Miami, FL 33199; Email: lassars@fiu.edu. FIU is an Equal Opportunity Employer and applications from minority candidates are encouraged.

325 WAYNE STATE UNIVERSITY invites applications for a tenure-track position at the Assistant or Associate Professor level beginning August 2007 to teach undergraduate and graduate (M.S.A. and M.B.A.) courses primarily in managerial accounting. Candidates must possess an earned doctoral degree in Accounting from an AACSB-accredited school. Applicants for an Associate Professor position must have a strong publication record and should be able to mentor junior faculty. The School actively encourages applications from women and minority persons. Rank/salary will be commensurate with qualifications and experience. WSU is a Carnegie Research Doctoral Extensive University located in Detroit, Michigan, with current enrollment of approximately 34,000, approximately 2,400 undergraduate and 1200 graduate business school students. Qualified candidates should submit their application packages electronically via our secure website at: http://jobs.wayne.edu, position number 033745 in the Department of Accounting in the School of Business Administration,. WSU is an Equal Opportunity/Affirmative Action Employer.

326 UNIVERSITY OF SOUTHERN MISSISSIPPI, Department of Accounting and Management Information Systems in the College of Business seeks applications for the position of Director of Accounting and Management Information Systems. Applicants for this position must possess an earned doctorate degree in Accounting, an earned doctorate in Management Information Systems, or an earned doctorate with at least 18 graduate semester hours in Accounting or Management Information Systems. Evidence of excellent collegiate teaching experience, scholarly and professional activities (i.e., in-field research publications) is required. Applicants should merit a tenure-track academic appointment at the rank of at least Associate Professor in one of the academic disciplines in the department. Interested candidates should submit an application, resume, and academic transcripts to: Search Committee, Dr. Barry J. Babin, The University of Southern Mississippi, 118 College Drive, #5091, Hattiesburg, MS 39406-0001; Phone: (601) 266-4627; Fax: (601) 266-4630; Email: barry.babin@usm.edu. The University of Southern Mississippi is an AA /EOE/ADAI employer and encourages applications from females and minorities. For more information, go to: http://www.usm.edu/colleges/cbed.

327 MARSHALL UNIVERSITY, Lewis College of Business (AACSB-accredited) invites applications for a tenure-track position in Tax at the Assistant/Associate level in the Division of Accountancy and Legal Environment available August 2007. A Ph.D./D.B.A. in Accounting is the preferred academic credential. Individuals with J.D./L.L.M. (Taxation) are encouraged to apply. Preference will be given to candidates with professional certification and/or significant taxation experience. Applicants must demonstrate a commitment to teaching excellence and conducting publishable tax research. Expectations include teaching undergraduate and graduate level courses in Taxation including Federal, State, and Local; Tax Planning; and Tax Research. Candidates should be capable of teaching in at least one other area of accounting. Please send a letter of application, a current *vita*, and names, addresses, and telephone numbers of at least three references to: Dr. Loren Wenzel, Division Head, Accountancy/Legal Environment, Marshall University, One John Marshall Drive, Huntington, WV 25755-2310. For further information please call: (304) 696-2660; or email: wenzel@marshall.edu. This position will remain open until filled.

328 SAINT LOUIS UNIVERSITY, a Catholic, Jesuit institution dedicated to student learning, research, health care, and service, is seeking applicants for: a tenure-track position in Accounting at the Assistant/Associate/Professor level, and a one-year visiting non-tenure-track appointment, both beginning Fall 2007. Candidates must demonstrate commitment to teaching excellence, quality research, and publication. Service to the department and university are required in addition to teaching and scholarship. Teaching interest is open; all areas, including accounting information systems and financial accounting, will be considered. A Ph.D. in accounting is required. Salary is competitive. Interested candidates must submit a cover letter, application, and current *curriculum vitae* to: http://jobs.slu.edu. Candidates are encouraged to schedule an interview at the AAA meeting in Chicago by contacting: Dr. Ananth Seetharaman, Accounting Chair, John Cook School of Business, 3674 Lindell Blvd., Saint Louis University, St. Louis, MO 63108. Saint Louis University is an Affirmative Action/Equal Opportunity Employer, and encourages nominations of and application from women and minorities.

329 KAPLAN UNIVERSITY, Graduate School of Management is looking for part-time instructors to teach accounting courses online. Pending budget approval, full-time teaching positions may come available in 2007. Internal postings precede external announcements for full-time appointments. Kaplan University is accredited by The Higher Learning Commission and is a member of the North Central Association (NCA). Kaplan University is part of Kaplan Higher Education. We are seeking faculty that value quality, teaching excellence, and want to make a difference in the lives of our students. Well-qualified applicants will have terminal degrees from regionally accredited universities in the subjects they wish to teach, experience teaching in their field, and professional experience in a nonacademic environment relating to their teaching area. Kaplan is committed to faculty support and offers benefits programs to both full- and part-time faculty members. Please submit a cover letter and *cv* to the Assistant Dean of Faculty at: ADFacultyGradManage@kaplan.edu.

330 ENDICOTT COLLEGE is a Baccalaureate and Master's degree-granting institution located on the North Shore of Massachusetts. Endicott enrolls more than 1,750 undergraduates and 1,000 Master's and Professional Studies students on its Beverly Campus. The College also has campuses in Mexico City and Madrid. Accounting Faculty: Assistant/Associate Professor Teaching faculty position needed to help develop and teach new CPA-track undergraduate program in Accounting. Open to candidates with teaching interest in any traditional areas of Accounting. Position includes academic advising of up to 20 students. Requirements include: Ph.D. in Accounting (preferred), but will consider ABDs close to completion or those holding a Master's degree with CPA and significant industry experience. Demonstrated effectiveness in teaching undergraduates, and ability to teach across a number of Accounting areas required. Prefer candidates with experience in use of computer technologies in teaching as well as expertise in curriculum development. Please send a letter of application, resume, and names and phone numbers of three references to: Human Resources, 376 Hale Street, Beverly, MA 01915; Fax: (978) 927-0084; Email: skoso@endicott.edu. AA/EKE.

UNIVERSITY OF DELAWARE.

One of the oldest institutions of higher education in this country, the University of Delaware today combines tradition and innovation, offering students a rich heritage along with the latest in instructional and research technology. The University of Delaware is a Land-Grant, Sea-Grant, Urban-Grant and Space-Grant institution with its main campus in Newark, DE, located halfway between Washington, DC and New York City. Please visit our website at **www.udel.edu.**

Assistant Professor - Taxation

Department of Accounting and MIS

The Alfred Lerner College of Business and Economics at the University of Delaware invites applicants for a tenure-track Assistant Professor position in Taxation beginning September 2007. Primary qualifications include outstanding research potential and teaching ability. Candidates must hold a doctorate in Accounting or be ABD with completion expected prior to joining. Assistant Professors with a few years of experience are encouraged to apply. Candidates are expected to teach undergraduate and graduate level courses. Salary, benefits, and support are competitive with AACSB accredited schools. We will interview applicants at the 2007 American Accounting Association meeting in Chicago, IL.

Qualified candidates should send three letters of reference, curriculum vitae, and summaries of teaching performance to Kent St. Pierre, Chair, Search Committee, Alfred Lerner College of Business and Economics, Department of Accounting and MIS, University of Delaware, Newark, DE 19716, 302-831-2961. Candidates may apply electronically by sending their application materials to warrenn@lerner.udel.edu. Review of applications will begin on August 1, 2007. The curriculum vitae and letters of reference shall be shared with departmental faculty.

The UNIVERSITY OF DELAWARE is an Equal Opportunity Employer which encourages applications from Minority Group Members and Women.

NANYANG
TECHNOLOGICAL
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A Global University of Excellence

Nanyang Business School, Division of Accounting

Faculty Position in Accounting

The Division of Accounting in the Nanyang Business School (NBS) at Nanyang Technological University, Singapore, invites applications for a faculty position in Accounting at the Associate and Full Professor levels. All research areas will be considered. Visiting positions are also available.

JOB QUALIFICATIONS:
Applicants should possess a PhD in accounting, and have a demonstrated commitment to research and teaching commensurate with rank.

ABOUT THE SCHOOL:
NBS offers undergraduate degrees in Accountancy and Business, MBA degrees and PhD degrees. Our undergraduate accountancy program is highly regarded in the region, and we collaborate with the University of Illinois (Urbana-Champaign) for our accountancy curriculum. NBS is accredited by AACSB and EQUIS; and our Accountancy programs have the AACSB accreditation in Accounting. Our MBA program has been ranked the best in Singapore and third in the Asia Pacific region in the recent Financial Times (FT)'s listing of the top MBAs in the world.

High quality research is strongly encouraged, and is evident from publications by our full-time accounting faculty in The Accounting Review, Journal of Accounting Research, Journal of Accounting and Economics and Contemporary Accounting Research in the past five years. In a 2005 article on the ranking of research productivity in Accounting, we are ranked the top business school in Singapore and the top 3 in Australasia. NBS also co-hosts the International Symposium on Audit Research, a collaboration with the University of Southern California, University of New South Wales, and Maastricht University.

NBS offers competitive salary and benefits in a stimulating research environment. A reduced teaching load is provided to new faculty and research-active faculty, and generous funding support for research and conferences is also available. The School also subscribes to various leading archival databases.

APPLICATION PROCEDURE:
To apply, please refer to the Guidelines for Submitting an Application for Faculty Appointment (**http://www.ntu.edu.sg/hr/faculty_guidelines.htm**) and send your application to: **Faculty Search Committee, Division of Accounting, Nanyang Business School, NANYANG TECHNOLOGICAL UNIVERSITY, S3-01B-51, Nanyang Avenue, Singapore 639798, Fax: (65) 6793-7956, Email: H-Dat@ntu.edu.sg, Website: http://www.ntu.edu.sg/hr/recruit/acad/nbs.htm**

THE HONG KONG
POLYTECHNIC UNIVERSITY
香港理工大學

SCHOOL OF ACCOUNTING AND FINANCE

Chair Professor of Accounting

The Hong Kong Polytechnic University aims to appoint a Chair Professor of Accounting to provide leadership in all aspects of academic activities, including teaching, research and consultancy in the School of Accounting and Finance, which is one of the three Departments in the Faculty of Business. Information on the School can be obtained through its departmental homepage at http://www.af.polyu.edu.hk/.

Applicants should have (a) a PhD degree in Accounting and solid academic background; (b) an international reputation in high quality interdisciplinary scholarship and research in any field of accounting; (c) a strong record in attracting research funding; (d) a demonstrated commitment to excellence in teaching; and (e) proven qualities of leadership.

Applicants are invited to contact Professor Ferdinand A.Gul, Chair Professor of Accounting and Corporate Governance, and Head of School of Accounting and Finance, at email afgul@inet.polyu.edu.hk for further information.

Initial appointment will be made on a fixed-term gratuity-bearing contract. Re-engagement thereafter is subject to mutual agreement. Remuneration package will be highly competitive.

Applicants are invited to send detailed curriculum vitae with names and addresses of two referees to the **Human Resources Office, 13/F, Li Ka Shing Tower, The Hong Kong Polytechnic University, Hung Hom, Kowloon (Fax: 2764 3374; E-mail: hrstaff@polyu.edu.hk). Recruitment will continue until the position is filled.** Candidature may be obtained by nomination. The University reserves the right not to fill this post or to make an appointment by invitation. General information about the University is available on the University' s World Wide Web Homepage http://www.polyu.edu.hk, or from the Human Resources Office (Tel: 2766 5306). Details of the University' s Personal Information Collection Statement for recruitment can be found at http://www.polyu.edu.hk/hro/jobpics.htm.

THE ACCOUNTING REVIEW
ISSN 0001-4826

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